A]

THE

JOURNAL

OF

ANATOMY AND PHYSIOLOGY

CONDUCTED BY

G. M. HUMPHRY, M.D. F.R.S.
PROFESSOR OF ANATOMY IN THE UNIVERSITY OF CAMBRIDGE,
HONORARY FELLOW OF DOWNING COLLEGE;

AND

WM. TURNER, M.B.
PROFESSOR OF ANATOMY IN THE UNIVERSITY OF EDINBURGH.

VOLUME V.
(*SECOND SERIES, VOL. IV.*)

MACMILLAN AND CO.
Cambridge and London.
1871.

Cambridge:
PRINTED BY C. J. CLAY, M.A.
AT THE UNIVERSITY PRESS.

CONTENTS.

FIRST PART. NOVEMBER, 1870.

SECOND PART. MAY, 1871.

Journal of Anatomy and Physiology.

ON SOME POINTS OF OSTEOLOGY OF THE PICHICIÉGO (*Chlamydophorus truncatus,* Harlan).

By EDWARD ATKINSON, F.L.S., *Hon. Curator in Zoology to the Lit. and Philos. Society's Museum; Lecturer in Botany, School of Medicine, Leeds, &c. &c.* Pl. I.

Read before the British Association at Liverpool, 1870.

THIS extraordinary little animal, the smallest of the *Edentata,* is so rare (not more, I believe, than ten[1] specimens having been captured or recorded since it was originally described by Dr Harlan of Philadelphia, from the then unique example in the Museum of Natural History in that city); and its literature, in this country, is so far from being complete, that I trust a few observations upon a perfect adult male, recently presented to the Leeds Philosophical and Literary Society, may not be deemed superfluous.

It may be well, first, to glance over the bibliography of this little quadruped. Harlan's original memoir was read before the American Phil. Soc. in January 1825, and published in the *Annals of the New York Lyceum of N. H.* This paper was reprinted *verbatim* in the 2nd Vol. of the *Zool. Journal* (London, 1825). Three years elapsed before a second specimen was secured in its only habitat—Mendoza, in the Argentine Republic—and transmitted

[1] The following is, I think, a correct list of all the recorded specimens :—
1. In the Mus. Nat. Hist. Philadelphia, 1825.
2. Sent to Zool. Soc. of London, 1828 (now in the British Musuem).
3. In the Mus. Acad. Sc. Berlin.
4. In the Acad. Sc. Buenos Ayres.
5 and 6. In the Mus. Univ. Halle (one imperfect).
7 and 8. In the Imp. Mus. Vienna (one immature, 1855).
9. In the British Mus. (imperfect), 1857.
10. In the Hunterian Mus. R. Coll. Surg. London (purchased at the International Exhib. 1862).
11. In the Leeds Phil. and Lit. Soc. Museum, 1869.

by Sir Woodbine Parish to the Zoological Society of London, who confided it to the late Mr Yarrell for preparation and description. The results of his investigations were published in the 3rd Vol. of the *Zool. Journal* (1828); but these, while adding comparatively little of importance to what Harlan had made known concerning the internal organization (for both the specimens described had been eviscerated), served rather to obscure than to elucidate the one marvellous feature of the skeleton, which, more than all the rest, distinguishes it, not only from other genera of its order, but from every known mammal, viz. the structure of the pelvis, with its anomalous bony shield[1]. Mr Yarrell, in fact, never recognized the shield as an integral part of the skeleton, but in his anxiety to preserve the skin with its 'coat of mail' entire, he actually *cut through* the five bony processes by which the shield is united to the sacrum and ischium; and congratulated himself on the feat, which although difficult, "was ultimately accomplished without injury" (!) The figure which accompanied his paper faithfully portrayed the mutilated processes, and served for the next twenty-seven years to mislead students of comparative osteology as to their true significance. Various authors, during this long interval, having no further opportunities of examining fresh specimens, were fain to repeat Yarrell's statements; and not few were the additional errors, which, either by careless translation into foreign languages, or otherwise, crept into their accounts. Even Cuvier speaks of the animal as possessing 10 teeth on either side of each jaw, while Harlan had correctly enumerated 8 only; and the same great man (on what authority I know not, unless it were from misunderstanding Harlan's plates—for it is evident he never saw the animal itself) speaks of the tail as being "partially attached to the underpart of the body."

It was not until 1855, however, that Prof. Hyrtl of Vienna rescued the *chlamydophorus* from its unmerited obscurity and misrepresentation. Having fortunately obtained a perfect adult female, sent to the Imperial museum in spirits, he made a most careful dissection of it, and published a beautifully illustrated

[1] Prof. Hyrtl *in loco*, p. 25.

and almost exhaustive treatise upon it[1]. This admirable monograph has never been translated in this country, and, curiously enough, is not even referred to by Dr J. E. Gray in his short paper "On the Structure of the Pelvis of Chlam. truncatus[2]," written in 1857 on dissecting a second (very imperfect) specimen sent to England by Sir W. Parish. Neither does Dr Gray include Prof. Hyrtl among the authorities quoted in his "Revision of the Genera and Species of Entomophagous Edentata," published in 1865[3]. Besides, so brief is the notice bestowed on the pelvic shield and other singular features of the skeleton in these communications, that English readers who may not have access to Prof. Hyrtl's treatise, will require no apology from me for availing myself of the Leeds specimen for its further illustration.

Our society is indebted to the kind exertions of Mr E. Harry Woods, of Rosario, and to Dr Day, of Mendoza, for this great acquisition. These gentlemen transmitted the animal in spirits, at the close of last year, when it was at once entrusted to Mr E. Gerrard, the well-known taxidermist, who with admirable skill has succeeded in preparing both a perfect skeleton and a stuffed skin from it. It was in order to secure this end that I reluctantly abstained from any previous dissection of the specimen. I now regret this mainly because Prof. Hyrtl's specimen being a female, I have lost the rare opportunity of examining the male generative apparatus *in situ.*

My present object, however, is to draw attention to several points in the skeleton, and first to the

HEAD. The general conformation of the head is very remarkable, differing from all other edentata in its relative dimensions[4].

From the following table it will be seen that, both in altitude and breadth, as compared with its length, the skull of the Chlamydophorus excels its congeners. I have not the opportunity of comparison with Xenurus or Tolypeutes, but I believe

[1] *Denkschriften d. Kais. Akad. d. Wissenschaften,* 9ter Band.
[2] P. Z. S. Part xxv. p. 8.
[3] P. Z. S. for 1865, p. 381.
[4] Yarrell's figure is *bad,* and has been invariably copied by all English authors, including Prof. Owen, in his *Comp. Anat. of Vert.* Vol. II. (1866).

they would both confirm my statement. The measurements are given *in lines.*

RELATIVE MEASUREMENTS OF EDENTATE SKULLS.

	Pholidotus javanicus.	Dasypus sexcinctus.	Orycteropus capensis.	Cyclothurus didactylus.	Bradypus tridactylus.	Chlamydophorus truncatus.
Height including Lower Jaw.	15	24	42	8	18	12
Breadth from Ear to Ear.	21	27	40	10	16	11
Length.	46	50	96	21	30	18

Thus it appears that the total height, in *Chlamydophorus*, from the base of the lower jaw to the vertex, and the diameter from ear to ear (not including the osseous auditory tube, which would add a line more on each side) are within one line of being equal; whilst the total length, from occiput to end of snout, is only once and a half as much as the height. A comparison with the other skulls measured shews no approach to these results.

The configuration of the skull, as a whole, is conical: the brain-case is sub-globose and capacious. If you imagine the remarkable frontal tuberosities and the deep lower jaw removed, you perceive a marked similarity between the crania of Chlamydophorus and Chrysochloris.

But the general facies, seen in profile, suggests, with the exception of the elongated lower jaw, a resemblance to the Asiatic elephant. This is mainly due to the elevation of the vertex, and the concavity of the *os frontis* (between the tuberosities), as well as to the inconspicuous orbit which is scarcely distinguishable from the zygomatic fossa, while the optic foramen is so minute as to be hardly visible.

The lower jaw is remarkable for its pachydermatous characters—its great depth, perpendicular ramus, rounded angle,

and the shortness of its coronoid process as compared with the condyloid. It bears, however, a not distant likeness to the lower jaw of the insectivorous *Macroscelides typicus*, which has also a short coronoid.

EAR. Our animal would appear, like the desman and the mole, to be devoid of an external ear, but this is not the case. True, there is no pinna, and the concha is represented only by a delicate cup-like ring of cartilage surrounding the aural orifice immediately behind the eye. But on removing the integument. a very singular auditory apparatus is displayed in the form of a long winding osseous tube, commencing where the bony meatus generally ends, ascending behind the articulation of the jaw, arching over the root of the zygoma, and then running horizontally to the point where the modified conchal orifice opens close to the tiny eye. This winding tube, however, is not in a single piece, but in two; the first of which is connected by cartilage with the short canal (the true bony meatus, not a quarter line in length) leading from the tympanic 'bulla,' and extends to a length of three lines, terminating at a point opposite the junction of the temporal, parietal and frontal bones. At this point it is joined by an interposed ring of cartilage to the second piece, only one line in length. This singular acoustic tube is capable of limited movement, and is furnished with a delicate muscle attached to the temporal bone near the root of the zygoma. The *tympanic bulla*, just mentioned, is in fact the tympanum itself, developed external to the cranium. It occupies the ordinary position of the mastoid process, consists of an ovoid, bubble-like, thin-walled chamber of considerable capacity—separated from the meatus by a true membrana tympani—and contains the usual chain of ossicles connecting that membrane with the fenestra ovalis. On its lower and inner aspect this bulla is somewhat contracted, and is pierced immediately behind the pterygoid plate by a minute orifice leading into the eustachian tube. To revert to the osseous tube connecting the tympanum with the concha, I would observe that it is evidently the homologue of the *cartilaginous* meatus auditorius, elongated and ossified to meet a special requirement. The nearest approach to this remarkable development is in the Echidna, whose external

auditory meatus is composed of a long series of cartilaginous rings. Resolve these rings into two bony tubes, and you have the organ of the Chlamydophorus.

The OLFACTORY organ has also a special development; for in addition to the usual surface afforded by the nasal fossæ and the ethmoidal cells, the two remarkable frontal tuberosities consist of an intricate labyrinth of cells, which freely communicate with the latter.

Hence it will be seen that although the organ of vision is so inconspicuous as to have earned for the animal the name of Pichi-ciégo, or 'blind armadillo'—the deficient sight is abundantly compensated by the extraordinary provision made for smell and hearing.

The *Scapula* attracts notice on several accounts. 1. From its general contour: the superior margin is convex, the inferior concave, and the anterior straight, while the posterior angle curves downward to a sharp point like a pruning-hook. In Dasypus the anterior and superior margins are convex, and the inferior straight, while the posterior angle points directly backwards. 2. In Chlamydophorus the dorsum is divided into three nearly equal portions by two spines[1] or crests, the upper of which supports the enormously long hooked acromion, and the inferior, not quite so elevated, terminates at the neck of the scapula. The space between the lower spine and the angle affords a broad origin for the powerful triceps muscle. 3. The acromion process, which is equal in length to the spine from which it springs, takes so wide a sweep as to overhang the greater tuberosity of the humerus; thus affording increased leverage to that bone, and while protecting the joint anteriorly, enlarges its cavity; this renders necessary the interposition of a synovial bursa[2]. A similar arrangement occurs in *Orycteropus.*

The *Sternum* presents one or two features worthy of note. It is composed of six bones, of which the first (manubrium) is much the largest, for the support of the broad articulation of the plate-like first rib. This bone and the next (first mesosternal) have a sharp crest or keel along their anterior surface—

[1] As in *cholæpus* and *myrmecophaga,* and some S. American *simiadæ.*
[2] Prof. Hyrtl, p. 23.

a vestige of bird-like structure which is seen in a less degree in *Cholœpus* and *Dasypus*, and in the 'interclavicle' of *Ornithorhynchus*[1]; but in *Chlamydophorus* this feature is proportionally more ornithic than in either of these. The next three pieces are small, like flattened beads connected by cartilage, thus serving, together with the articulated ribs (another bird-like element) to compensate for the want of expansibility, owing to the complete ossification of the latter. The xiphisternum is separated from the fifth piece by the dovetailing-in of the conjoined plates of the 6th, 7th, and 8th ribs on either side.

But by far the most striking anomaly in the skeleton before us is the singular pelvic shield, to which Hyrtl has given the name 'sphæroma ischii.' As his description is quite exhaustive, I cannot do better than transcribe it. He begins by exclaiming with pardonable enthusiasm: "Inusitata, vel, ut rectius dicam, inaudita plane pelvis conformatio, verum prodigium osteologicum est, cui per omne mammalium regnum, nihil simile datur aut secundum."

He then proceeds: "The unusual characters (of the pelvis) consist, first, in the pubis being open in front, as in birds; and secondly, in the mutual growing together (mutuo coalitu) of the enormous tuberosities of the ischium, whence is developed the truly miraculous bony shield, which for the future it will be convenient to call the *Sphæroma ischii*.

"The pelvis, elongated as in the *Dasypi*, is composed of the sacrum and innominate bones, whose symphyses degenerate into true synostoses. The number of the sacral vertebræ, on account of their intimate union, can only be estimated by the number of inferior sacral foramina, which, since there is some asymmetry in the anterior region of the pelvis, appear to be ten. The first three coalesce with the iliac bones—the last four blend with the ischia—the three middle contract no union with either of these bones, but constitute the upper margin of the ischiadic foramen. Thus far it tallies in the strictest sense with *Dasypus gymnurus* (Xenurus, Rapp). But now appears a notable difference. From the dorsum of the sacrum arise three longi-

[1] Parker's *Monograph on Structure and Development of Shoulder-Girdle*, p. 193.

tudinal crests, of which the median absorbs the lateral ones, so that behind the great ischiadic foramen only one crest remains, which, in the shape of a slender, pellucid, and much-perforated lamina, is produced throughout the length of the sacrum, and posteriorly amalgamates with the sphæroma. This median crest, at the spot where, by the accession of the lateral ones, it receives an increase of substance, is evolved into a dense *transverse lamina*, which is associated on either margin with a long and unusual process of the ischium that I denominate the *ascending*. By this means a double canal is formed, whose arch is the transverse lamina just described—whose base is the dorsum of the sacrum—whose external wall is the ascending process of the ischium, and whose internal wall is the median crest itself which separates the two canals. These ample canals we will call *c. suprasacrales majores*. Nor is this all. From the dorsal aspect of the transverse lamina are sent out, in a backward and upward direction, two strong rounded bony columns, which, diverging from each other, impinge upon the sphæroma and become as many props to it. It is evident that between these columns and the median crest, there are two canals also, similar to those already described. These we name the *c. suprasacrales minores*."

In the adult *D. gymnurus*, the innominate bone consists, as usual, of three bones which are joined by interposed cartilage. But in *Chlamydophorus* they form a genuine synostosis.

The *ilia* surmount the sides of the anterior sacral vertebræ, with which they are united, in the shape of wings, and afford an ample surface for the origin of the dorsal muscles. The ischiadic notch is converted into a true foramen.

The *ischium* is united to the sides of the posterior sacral vertebræ, and possesses three rami, of which the superior is the 'ascending process' above named; the second, directed downwards and forwards, meets the os pubis, with which it unites to form the lower boundary of the obturator foramen; the third, remarkable for its strength and breadth, affords the sturdiest support to the sphæroma ischii, into which it is inserted.

The *os pubis* is distinguished from the ilium by the presence of a broadish ilio-pubic tubercle. It consists of only one ramus which is joined to the descending ramus of the ischium,

and closes the obturator foramen. With its fellow of the opposite side it has no connection, and hence, in the bony pelvis, there is a gap below of six lines in width[1]. In place of a symphysis, however, there is an arch of fibro-cartilage projected forwards which closes the pubes, and receives the fibres of the *recti abdominis* muscles. In *Xenurus* (according to Hyrtl) the symphysis is closed, not by the continuous development of the pubic bones, but by the interposition of a small wedge-like bone on either side of the median line. Similar cuneate bones are said to occur in *D. sexcinctus*, but not in *D. novemcinctus*[2], where the normal ossification is complete. This is a very interesting developmental link.

This marvellous Sphæroma, then, distinctly belongs to, and is part of, the bony pelvis, to which it is united by five *fulcra*. Of these the two principal, and lowest in situation, arise from the ischium—the two middle are the columns erected on the transverse lamina of the sacral crest—and the highest is the termination of the median crest. Since the main buttresses of the shield occupy the place belonging, in other Dasypods, to the *tubera ischii*, it seems evident that this anomalous structure is developed from the confluence of the tuberosities. It has no analogy to the cutaneous skeleton which occurs in the form of bony plates in all the Dasypi: for in these animals the bony shields of the skin are never united to the subjacent parts of the skeleton by a true synostosis, as in Chlamydophorus.

In position, the sphæroma is vertical; in shape, it may be compared to a semicircular buckler with its convex margin directed upwards. This margin is thick, rounded, and beautifully crenated with bosses and intermediate fissures, corresponding with the first row of tesseræ in the perpendicular piece of the horny dermal coat which overlies it. The lower margin of the shield is acute, and also crenated, and consists of two nearly straight radii, as it were, converging to the *semilunar median* notch, which embraces the base of the tail, but without touching it. The anterior surface is concave, but unequal and rough, pierced by many nutrient foramina. The posterior is smooth,

[1] This hiatus is greater in proportion than in the Sloths or in the Shrews the only other mammals whose pelves are open anteriorly.

[2] *Tatusia peba*, Desm.

but bossed and ornamented with semicircular rows of oblong foramina and fissures, arranged concentrically, which give insertion to the short tough fibres, by which the dermal coat is bound to the shield throughout.

I have preferred to use the term 'dermal coat' (chlamys) to indicate the tesselated horny covering, which clothes the top of the head, back and tail of the animal, as well as the bony pelvic shield; because some confusion is introduced by previous writers[1] by the interchange of the terms 'dorsal disk' and 'dorsal shield' —'pelvic disk' and 'pelvic shield'—whereas in Chlamydophorus there is (in the dorsal region) *no* shield as distinct from the horny disk or coat. Neither in the Dasypods, which have bony exoskeletal shields, is there any endoskeletal pelvic shield.

With respect, however, to the relation between the sphæroma ischii and the cutaneous system, it must be observed that the former is completely invested, on *both* its surfaces, by the common integument of the body: so that the anterior concavity and the bony *fulcra* implanted into it are clothed with *hairy skin* reflected from the back and nates; while the posterior surface is covered by the closely adherent continuation of the dermal horny coat. The hairy skin which surrounds the fulcra of the sphæroma insinuates itself even into their interstices, whence the sinuses are formed which Yarrell mistook for anal glands[2].

Before quitting this portion of the subject, we may enquire what is the purpose of this singular truncated extremity so strongly fortified within by bony shield and buttresses? At first sight it appears rather obstructive than calculated to serve any useful end: but on considering what little is known of the habits of the animal, it will be evident that it fulfils a most important office as a defensive weapon—the only one the little creature possesses; while the obstruction which it might appear to offer to the performance of sexual functions, is compensated by the special structure of the male and female organs. The Armadillos live above ground, and, when attacked by birds of prey or other enemies, are able to roll themselves into a ball, in which position the overlapping plates of their osseous armour

[1] Gray's *Catal. Edent. Mamm.* p. 388, and elsewhere.
[2] *Zool. Journal*, Vol. III. *loco cit.*

afford them their necessary protection. The haunt of the Pichiciégo, however, is its subterraneous burrow, whither on the slightest alarm it betakes itself, and from which it never wanders far. Hence it does not require a stronger armature for its head and back than what is sufficient to support the superincumbent loose sandy soil through which it digs its way; while its sturdy pelvic shield covers its retreat by effectually closing the orifice, much on the same principle as a gasteropod shelters behind its operculum. Thus its nimblest foes, which probably are serpents, may be eluded; and, as if to give them "no handle to get hold of," the animal is able to curl up its tail closely against the abdomen, and so complete this admirable defence.

From Hyrtl's description of the female organ, which I give entire below, it appears that the orifice of the vagina is situated above the pubes, but is prolonged into a tube a quarter of an inch in length[1], and, as I gather from his expressions (though he does not distinctly say so), directed *backwards*, so as to facilitate copulation. On the other hand Yarrell, whose specimen was a male, speaks of the penis, which "remained attached to one edge of the divided abdominal muscles," as being "large compared to the size of the animal, and one inch and one eighth in length." In our specimen the length was about the same, of which one quarter-inch was free externally, and semi-pendulous. These facts taken together seem to render unnecessary the possible hypothesis suggested by Prof. Hyrtl, as to the *modus coitûs*, but in which he himself evidently does not believe.

The Mammæ[2] are pectoral, and two in number. The testes are abdominal, but I am unable to speak definitely as to their exact situation.

[1] "Vagina insolitæ formæ organon, non uti regula fert, ostio ante anum hiante, exordium sumit, sed tubum sistit, supra inguen elevatum, cylindricum, ad tres lineas longum, extus pilosum, intus membrana mucosa, in ostii externi vicinia plicis longitudinalibus ornata, obductum, ita ut Priapum potius quam sinum pudoris feminini ante oculos habere crederes; et ipsemet ego, primo animalis aspectu, cum masculo rem mihi esse, hallucinaverim.

"Forsan pelvis conformatio singularis, amplexibus venereis more ferarum a posteriori celebrandis, minime favens, talem vaginæ in tubum cylindricum prolongationem necessariam reddit, ut vagina hoc pacto peni, in venerem ruenti, quasi obviam venire possit. Si vero, quod serioribus observationibus dijudicandum et statuendum relinquimus, Chlamydophori femina, copulam sexualem adverso ventre inire soleat, revera plane non intelligendum erit, cui officio vaginæ priapiformis longitudo inserviat." *Op. cit.* p. 50.

[2] *Idem.* p. 51.

But to return to the skeleton, there are yet several points on which I would say a few words. And first, as to the *teeth*—for here I find my observations do not quite accord with those either of Prof. Hyrtl, or of Dr Harlan. I will, however, for brevity's sake, not quote these writers, but simply describe from our own specimen.

The Chlamydophorus is a true homodont, with eight grinders on either side of both maxilla and mandible. The teeth are long, subcylindrical, separated by intervals, and deeply set (those of the lower jaw perforating the whole depth of the bone, and dimpling its inferior margin); each tooth is nearly equal in diameter throughout its length, and has therefore no true crown or root. They are slightly curved[1], so that each tooth with its antagonist taken together describes an arc of 25° (see fig.) with the convexity forwards.

Before speaking of the opposed surfaces, I would draw attention to the articulation of the jaw. It is plain that the condyle, being so closely packed in between the root of the zygoma and the external ear-tube, can have very slight antero-posterior movement: hence, no single tooth can be brought into contact with any other but its proper antagonist.

The first tooth of the lower jaw has no opponent and therefore no masticatory surface. It is rounded at the apex, and in our specimen (as in Hyrtl's) is the smallest of all [while Yarrell, whose specimen was perhaps older, describes it as the longest, as we might naturally expect it it to be]. The first seven teeth of the upper jaw, then, are opposed to the last seven of the lower; and hence the eighth upper tooth is also without an antagonist, but, unlike its analogue in front, it has a double facet.

In the upper jaw, the first five teeth have a single oblique plane facet, directed backwards, while the remaining three have two equal facets each, converging to an angle. In the lower jaw, the 2nd, 3rd and fourth teeth have single plane facets cut obliquely to correspond with the 1st, 2nd and 3rd of the upper; in the 5th and 6th the posterior plane is slightly developed; while the 7th and 8th alone have the anterior and posterior planes equal.

[1] Hyrtl describes them as curved, but his plate represents them straight.

From this arrangement it will be seen that very slight retraction of the jaw would lock the teeth, and hence the lateral movement must prevail in mastication.

The *os hyoides* has been remarkably well preserved in our specimen, and as it is fully described by Hyrtl, I will give his own words:

"The body (basis), curved in the form of a horse-shoe, presents a median tubercle for [the attachment of] muscles coming from the sternum. There is no entoglossal bone. Neither in Chlamydophorus nor in D. sexinctus have I seen the process of the body reaching to the thyroid cartilage, which Rapp ascribes to the Dasypi. The *posterior* horns, which in man are called 'c. magna,' are only small tubercles, joined by a genuine synostosis to the body of the hyoid bone. They contract a true articulation with the anterior cornua of the thyroid cartilage, which occurs in the Dasypi. The *anterior* horns of the hyoid (in man, 'c. minora') exceed in length the body itself, and are composed of three segments. The first is the shortest, but thicker than the rest, and joined to the body by arthrosis—not by sychondrosis, as authors erroneously suppose. The second segment is the longest, and somewhat terete; it coheres with the first and third by means of capsular ligaments, and not by interposed cartilage. The third segment is a rudiment of the styloid bone, and is closely connected by tendinous fibres to the petrosal."

With regard to the natural position of the tail of the animal there appears to be some doubt. Its length from the notch in the sphæroma to the apex is an inch and a quarter; it is somewhat expanded and flattened towards the end, into the shape of a spatula. Above it is covered with scales similar in consistence to the *theca dorsalis*—below it is smooth. Harlan describes and figures it as permanently curved under, and lying close against the belly, except when used, as he supposes, for a paddle. Yarrell adopts the same view (though he represents the tail *extended* in his figure of the skeleton) and enlarges on it, believing that it is "probably exercised in removing backwards the loose earth accumulated under the belly by the action of the fore legs" when burrowing. Prof. Hyrtl admits that he is puzzled. For though in both his specimens the

tail was bent forwards, and could not be reversed[1]—yet he could not believe this position was natural, because of the impediment it must offer to the animal's progress underground. In our specimen the tail was extended when it came to us, and it was evident the animal, which had been captured alive and kept some time in captivity, had died with the tail in that position. Mr Gerrard says, "I tried to make it curve under the body, but it would not—so I left it as I found it;" and he adds, "I do not think it would be found to curve in the opposite direction after death to that whilst living, even allowing for the contracting power of strong spirit." In all probability Harlan's and Hyrtl's specimens were taken while retreating to their burrows and killed at once, the tail preserving the attitude which it assumes under those circumstances—its natural position, when the animal is unmolested, being that of extension.

I venture to think, too, that another point remains in doubt regarding our little animal—namely, its mode of progression: and if, as I believe, it can be shewn that the Chlamydophorus, like *Myrmecophaga jubata*, and the *Manididæ*, walks on the *dorsum* of its fore claws, its position in the order will be changed. It is said that *Priodon gigas* has a similar manner of gait, which, if true, will render my supposition more probable; inasmuch as the latter is not only generically nearer to our animal, but the structure of the toes in Priodon and Chlamydophorus is more nearly alike by far, than is that of either of the first-named genera to these.

First, then, our specimen, when it arrived, had the toes of the fore feet doubled inwards with their claws resting upon the palms, nor could they without violence be got to lie flat. This, it may be said, was due to the strong spirit in which it had been kept: but (as already observed of the tail) this did not seem sufficient to account for it. Secondly, the appearance of the palms, of which the radial half is covered with soft fur, and therefore is unfitted for contact with the soil. Thirdly, the flattened shape of the claws, and their imbricated arrangement, *i.e.* the 5th overlapping the 4th, and this overlapping the 3rd,

[1] "... versus ventrem infimum inflexa, tendit, neque majore vi facile mutatur."

would, when strongly flexed, present a continuous flat surface on which the weight of the fore limb could rest. Add to this, that the dorsal aspect of the claws had the appearance of having been used in this way. Having observed these points as likely to account for the actual position of the fore feet in our specimen, I was struck with Prof. Hyrtl's description of the muscular structure of the palm—the more so as he does not suspect the mode of progression to be different to that of *D. gymnurus*, with which he compares it. When describing the flexor muscles of the fore limb, he thus expresses them: "A singular union characterizes the flexorial province of the muscles of the forearm and hand, whose number is less than usual from the absence of the *flexor longus pollicis* and *pronator quadratus*, but whose fleshy belly presents so large a mass as to strike one with admiration. With so rich an apparatus of muscles has the sagacity of nature provided for the flexion of the hand and fingers—strength being combined with celerity of action—that the prompt flexion of these may occur at the same moment, and with the same effort as extends the forearm......These muscles have a common fleshy origin, consisting of three layers. The superficial layer yields to the rest in volume, and divides into four tendons, of which the *first, second* and *fourth* resemble in their course and mode of insertion the *pronator teres*, and the radial and ulnar flexors of the carpus respectively. Concerning the *third* there appears some doubt. From its origin, course, and aponeurotic expansion you would take it for a *palmaris longus*. But when its palmar aponeurosis splits up into four flat tendons, which go to the first phalanges of the fingers (the pollex excepted), and there, by a forked division, give passage to flexor tendons which are produced to the last phalanges, the obvious suspicion is that you have to do with a *flexor digitorum sublimis*. I myself adopt the former view, and for the following reasons. Firstly, because the so-called *flexor sublimis* constitutes the mass of the second muscular layer. Secondly, because the common rule among mammals demands that the bifid tendons of the fl. sublimis should be inserted, not into the base of the first, but into the sides of the second internode. Thirdly, it is evident from an accurate examination of their insertion, that the tendons of which we speak are not perforated, but are

so blended with the fibrous sheath which guards the flexor tendons as would require even in the human hand a most subtle preparation to demonstrate. We therefore declare this ambiguous muscle to be the *palmaris longus*. Its tendon where it crosses the carpus contains a minute lenticular ossicle."

Hence, I think it will be clear that the unusual development of this muscle and its aponeurosis in Chlamydophorus is specially provided for fixing the palm in the flexed position, and thus enabling the animal to walk on the dorsum of its outer toes: while at the same time this attitude of the limb appears the best adapted for scooping out and throwing backwards the sand in which it burrows.

I am indebted to the valuable assistance of my friend, Mr James Walker, who has kindly prepared the drawings which accompany this paper.

DESCRIPTION OF PLATE.

Fig. 1. Skeleton of *Chlamydophorus truncatus* (nat. size). The *fulcra* supplied by the ischium are best seen in this figure.

Fig. 2. *Sphæroma ischii* and tail, viewed from behind (nat. size).

Fig. 3. Pelvis and sphæroma viewed from above and in front, so as to shew the *fulcra* supplied by the sacrum and the 'ascending processes.'

Fig. 4. The Sternum, clavicles, and first pair of ribs (nat. size).

Fig. 5. The external auditory apparatus, or 'acoustic tube.'

Fig. 6. The os hyoides. (Figs. 4 and 5 are twice nat. size.)

I regret to find that, in the transference to the stone by the lithographer whom I employed, the drawing of the teeth has been altered so that they all appear level-topped. Unfortunately this was not discovered till it was too late to be rectified.—Ed.

ON CARDIOGRAPH TRACINGS FROM THE HUMAN CHEST-WALL. By A. H. Garrod, *St John's College, Cambridge.*

On applying the hand over the left pectoral region the movements of the heart can be felt with facility, especially at the end of expiration. In the following paper an attempt is made to classify and partly explain these movements, as they are reproduced by the sphygmograph.

The earliest and perhaps the only published observations on these curves are by Dr Marey of Paris[1], who gives one trace from the human subject and others from the horse, which latter have the advantage of being associated with synchronous traces from the interior of the ventricle and of the auricle. No previous observations can be found as to the relative duration of the different elements of and the other peculiarities in the human heart apex traces at different rapidities of pulse.

While the subject is sitting or standing the sphygmograph can be made to give a very perfect record of the heart's movements, as they are transmitted to the intercostal tissues, by holding the instrument horizontally with the watchwork to the right hand, the plane of the recording paper and consequently of the whole instrument being parallel to the floor, and the lever-pad at or near the point of maximum pulsation, between the fifth and sixth ribs. While lying, the instrument must be held upright, as when wrist traces are taken.

The movements of respiration cause so much irregularity in these traces, that it is advisable to stop breathing while they are being taken; and this should be done at or near the end of a normal expiration; it is then found that little or no effect is produced on the heart's action, during the short time, about seven seconds, that the instrument is applied.

It will also be found that quick pulses are more easily taken than slow ones, because the heart can only be rendered slow by means that make the skin cold and inelastic, or by

[1] *Physiologie Médicale de la Circulation du Sang.* Paris, 1863. Pp. 68 and 121, and elsewhere.

positions that make the application of the apparatus more difficult.

In all cases the spring carrying the pad should be screwed down so as to give its greatest pressure.

In the account of the traces thus obtained, the rapid beats will be first described; after these the slow ones, by which means an idea can be best formed of the relation between curves at first sight so different as those produced when the heart's action is over 100 and those when it is below 50 in a minute (compare Figs. I. and VI.).

There is a great similarity in traces from pulses above 105 to those over 140 in a minute; and the description of one will include them all. Figure I. is from a heart beating 125, and it represents all the characteristic features. The movements of the lever are very extensive and sudden, so as to give the impression that they depend more on its momentum than on the heart's action; but that such is not the case is shewn by applying the instrument a little way from the point of greatest pulsation, when (as in Fig. II.) all the same elements appear, though much less ample and otherwise modified.

The main ascent commences abruptly immediately after a slight rise and fall (*a*, Figs. I. II.), and is always broken about midway (*b*) by a small fall; it is followed by a most considerable and rapid descent, which carries the lever in an unbroken line, down to a point almost as low as that from which it started. Subsequently to this comes a less sudden rise (*f*), which reaches about as high as the break in the main ascent; its summit is not nearly so sharp as the previous one, and from it a fall, frequently a little irregular, commences slowly, becoming more rapid, though it is interrupted by a slight rise (*g*), after which it continues to sink until it reaches the lowest point of the trace, from which it makes a sudden slight ascent (*k*), which soon becomes more gradual, continuing until the rise (*a*) from which the description commenced.

Neglecting for the present small differences in the relative durations of these components, the pulse of 140 differs from that of 110 a minute in the movements being more extensive and consequently the angles more sharp, the intermediate rate is being intermediate in character.

In the pulse of about 90 a minute (Fig. III.) another small rise and fall appears (*e*) in addition to those previously described, which is very constant, and becomes more considerable when the heart's action is slower.

Here also, as shewn in Fig. IV. in a trace taken directly after Fig. III. between the sixth and seventh ribs, there is sometimes seen a reduplication of the first part of the main rise (*b*, *c*, Fig. IV.), which is disguised in Fig. III. probably by the momentum of the lever. Another point in which it differs from the quicker pulses is in the formation of a second undulation (*b*) in the main descent before it reaches its lowest point. The main ascent also is not so extensive.

When about 70 beats are made in a minute the main rise can frequently be shewn to be doubly broken (*b*, *c*, Fig. V.); but these often get merged into one curved line. The subsequent fall (*d*) is here seen to have become much diminished, and the next rise and fall (*e*) of greater duration.

In the slow pulses (Figs. VI. IX.) the fall after the small rise preceding the main ascent (*a*) is inconsiderable, or nil, which makes that rise appear as part of the main one, which is not the case.

The rise *c* has now become more marked, while *d* has diminished so much that it is no longer the highest point of the trace, that now being at the end of the rise preceding the main descent (*f*), which is frequently found to be double.

It is to be noticed that as the pulse gets slower, the generally ascending line between *k* and *a* gets longer; also that at all rates there is a great similarity in shape in the fall and rise between the points *h* and *k*, which is quite characteristic of that part of the curve.

A precise knowledge of the causes of these various changes in the direction of the human apex trace will always be somewhat deficient, from the impossibility of vivisectional verification, and from the fact that the relations of the organs concerned is different in man to what it is in animals, from which, otherwise, arguments from homology might have been more extensively employed.

By means of synchronous traces from the exterior and interior of the heart of the horse Marey explains his apex trace,

which in the main resembles that from the human subject. He shews that the rise, here called *a*, results from the contraction of the auricles, which makes it clear that that event occurs much nearer to the commencing ventricular contraction, represented by the origin of the main ascent, than is supposed by many. He also shews that the semilunar valves close at the break, single in his trace, in the main descent. The irregularities in the systolic interval he considers due to vibration of the blood caused by the tightening of the auriculo-ventricular valves, but his results were recorded after having been communicated to india-rubber tubes filled with air, and the undulations probably originating in them.

It is necessary in attempting to explain these traces, especially when comparing different rapidities of pulse, always to bear in mind Marey's most important law, that "the arterial tension (blood potential) varies inversely as the rate of the pulse." This law, though disputed by some, must closely approximate to the truth, because by it so many facts with regard to the circulation of the blood are perfectly explained, that cannot be in the least accounted for elsewise; and it will shortly appear how much it assists in interpreting the curves under consideration.

After the auricles have contracted at *a*, the commencing ventricular action originates the main rise, which continues uninterrupted until the closure of the mitral and tricuspid valves at *b*. From this point, until the opening of the aortic and pulmonary valves, the heart's force is expended in raising the potential of its contained blood to that of the large arteries, and it is a well-known fact that during that time the form of the ventricles becomes somewhat globular, their diameter increasing and thus causing them to recede in their conical pericardial cavity, producing the fall *b* in the quick pulses.

Immediately the semilunar valves get opened the ventricles distend the proximal parts of the large arteries, and it is evident, from what has been said above with regard to the relation of blood potential and rapidity, that the quicker the pulse the more relaxed is the aorta at the moment before the semilunar valves open; consequently the more rapid the pulse the greater is the disturbance of equilibrium when they do so;

and as the aorta gets stretched and lengthened by the sudden repletion, so it sends the heart forward at that moment, causing the rise *d*, which must therefore be greater as the pulse is quicker, which is the case. In very slow pulses the blood potential being high, the repletion of the already greatly distended arteries does little in further filling them, but acts by sending the whole mass of blood forward, consequently the rise *d* is inconsiderable. The rise *c*, if not resulting from the shock of closure of the auriculo-ventricular valves, must remain unexplained.

The repletion of the proximal arteries is very rapid; and the accompanying rise is overcome in quick pulses by the speedy retreat of the apex, resulting from the emptying of the heart, causing the fall *d*, at the end of which the ventricles cease contracting.

In slow pulses the heart's systolé is prolonged, causing slight irregularities in the upward tending trace, which are fairly constant (*e*, which is frequently double).

There is evidently an appreciable interval between the end of the ventricular systolé and the closure of the semilunar valves, during which the retrograde blood-current is arriving at sufficient velocity to enable them to act; but if Marey's law of the relation of blood potential and rapidity of heart's action is correct, we are justified in going much further, and saying that *the quicker the pulse, the more slowly do the aortic valves close:* for, the greater the blood potential, the sooner does the heartward current become sufficiently rapid to close the valves, whose hydrodynamical relations are not otherwise modified by the rate.

From these considerations, combined with the fact, in quick pulses, that just before the rise *f* originates the trace loses its jerky character, it is most probable that the ventricles cease contracting just before the commencing rise *f* (at the end of the fall *d* in Figs. I. II.), and that the whole of the time occupied by the rise and fall *f* is employed in generating the retrograde current to close the valve, the change in direction of the curve being produced, first by the relaxation of the heart causing it to advance, and then by the partial collapse of the aorta causing it again to retire. This rise and fall is also clearly shewn in Fig. II. *f*.

It can be seen in Fig. IV. that in the pulse of 90 the undulation f is not so long as in the quicker ones, and in the slow curves it is shorter still, but the want of sharpness and the blending of the neighbouring rises prevent any accuracy being attainable in the latter cases.

The immediate effect of the closure of the semilunar valves, at the end of the fall f, is to cause a check to the descent of the lever (g), as the aorta is no longer emptying itself heartwards; but this is very soon counteracted by the consequent repletion of the coronary arteries[1], which, as can be easily shewn on the post-mortem heart, increases the diameter of the ventricles and makes them recede, drawing the apex back, further than during any other part of the revolution. During the rest of diastolé, other minor forces come into play which are not easy to trace.

Figures VII. VIII. IX. are given to shew that under different conditions the various rises and falls may be made to assume different degrees of importance. In Fig. VIII. where the greater part of the weight of the heart rests against the chest-wall, it is particularly to be noticed that the fall after g, before which the semilunar valves close, commences from the very top of the trace, shewing that the main force by which the heart is made to recede, which from the great length of the down stroke must be considerable, does not commence until after the closure of the aortic valve, which supports the theory of the cause of the active ventricular diastolé noticed above.

A superficial examination of cardiograph tracings is sufficient to convince the observer that when the heart beats slowly the *first* part of the revolution, namely, from the commencing systolé until the closure of the semilunar valves, bears a smaller ratio to the whole than in quick pulses. This led the author to make a series of measurements of these ratios, on the assumption that the ventricles commence to contract at the origin of the main rise, and that the semilunar valves close at the end of the fall f.

[1] That the active diastolé of the ventricles results from the congestion of the coronary vessels was discovered by Brücke; and I regret that my ignorance of his observations, published in *Sitzungberichte der Wiener Akad. der Wiss.* Nov. 1854, vol. XIV. p. 345, prevented my referring to them, in a paper on the same subject, in this Journal for May, 1869.

To ensure accuracy, the trace was placed on a flat piece of wood, to which was attached a ledge, along which it could be made to slide. A lever with a steel point was also in connection with the instrument, in such a way that when the tracing rested on the ledge, the steel point produced scratches on the paper, similar to those produced by the sphygmograph pen.

By this means, the parts of the curve under consideration can be all projected on to one straight line and their relative lengths measured with facility. Fig. x. is a trace so prepared for measuring, and this arrangement is necessary on account of the sphygmograph lever moving in part of a circle instead of quite vertically. Further, to diminish inaccuracies in the watchwork movement all the pulsations on a trace were measured, and their average taken as the result.

It was soon found that, with a given rapidity of pulse, the ratio of the *first* part of the heart's revolution to the whole did not vary appreciably, when traces were taken in any given position, but that when standing or sitting the *first* part was longer than when lying.

Further, on comparing traces of different rapidities, it was found that the length of the *first* part varied very definitely, inversely as the rate; not so quickly, but as its square root: and the number of measurements that have been made seems to justify the law, that *in health, the length of the first part of the heart's beat varies, for a given position of the subject, inversely as the square root of the rapidity.*

This result differs from that of Donders[1], who found that the length of the *first* part did not vary with different rates of heart's action; but his means were much less efficient, he having to depend on the registration by the hand of the first and second cardiac sounds.

All the facts on which the above law is supported are given in the accompanying table, in which they are thrown into the co-ordinate form, one co-ordinate, x, representing the rate of pulse, and the other, y, expressing the number of times the *first* part of the revolution is contained in the whole. The

[1] "On the Rhythm of the sounds of the heart." By F. C. Donders. 1865. Translated into the *Dublin Quarterly Journal of Medical Science*, Feb. 1868

observations made while lying are represented by a cross (×), when semi-recumbent these are encircled (⊗). The encircled dots (⊙) indicate that the position was sitting, and the standing ones are erect encircled crosses (⊕).

The simple dots are either from sitting or standing observations, but it is not certain which, as note was not taken at the time.

For example, the measurement of two heart traces, one at 41 a minute while lying, and another at 141 when semi-recumbent, gave in the former case the ratio of the *first* part to the whole revolution 1 : 3·4125, in the latter, 1 : 1·832; the length of the *first* part is found by multiplying the rate into the number of times the *first* part is contained in the whole, or the x by the y, when $\frac{1}{xy}$ is the required result.

Thus $41 \times 3{\cdot}4125 = 139{\cdot}9125$, say 140;

$141 \times 1{\cdot}832 = 258{\cdot}312$;

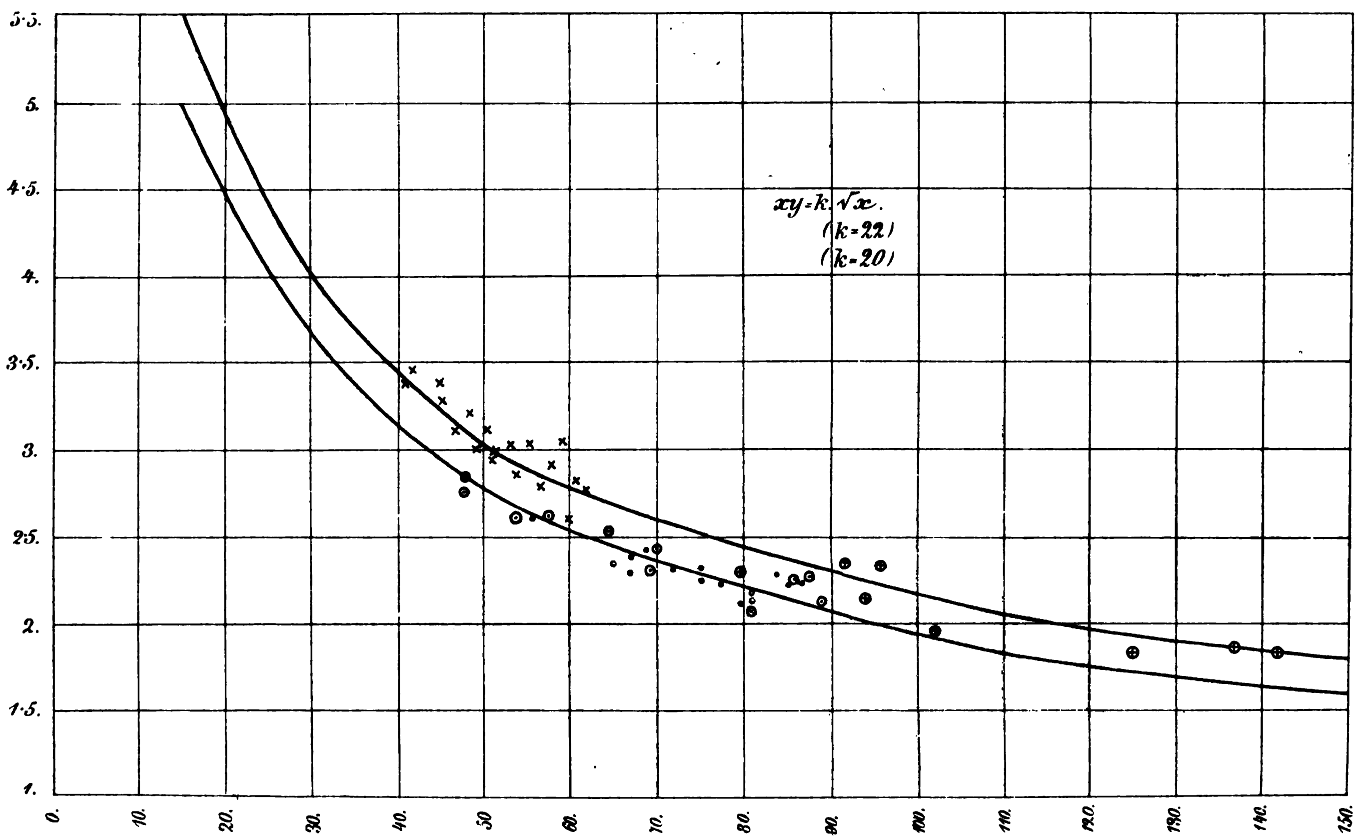
and $\sqrt{41} = 6{\cdot}4$, about; $\sqrt{141} = 11{\cdot}88$, about;

and $6{\cdot}4 : 11{\cdot}88 :: 140 : 259{\cdot}9$, about;

which is very near the required numbers.

If xy varies as the square root of x this can be stated thus, $xy = k\sqrt{x}$; k being a constant for any given position. Taking one of the above cases, x equalling 41, $xy = 140$; consequently $k = 22$ nearly, which includes, within the limits of experimental error, all the measurements that have been made of traces taken while lying. When sitting or standing the results are not quite so uniform, as may be seen in the table, on which the equation curves have been drawn with $k = 22$ and also $= 20$: the latter, the lower one, passes through or approaches most of the sitting and standing observations.

To find the duration of the ventricular systolé is very important, but not at all easy in many cases. In all rapid pulses, measuring from the commencement of the main ascent to the point from which the rise f originates, and which can scarcely be anything but the termination of the systolé, it has been found that that interval is contained 3·4 times in each revolution. In Fig. IV. the trace of 87·5 a minute (rare from being very detailed), it is found that between the same points

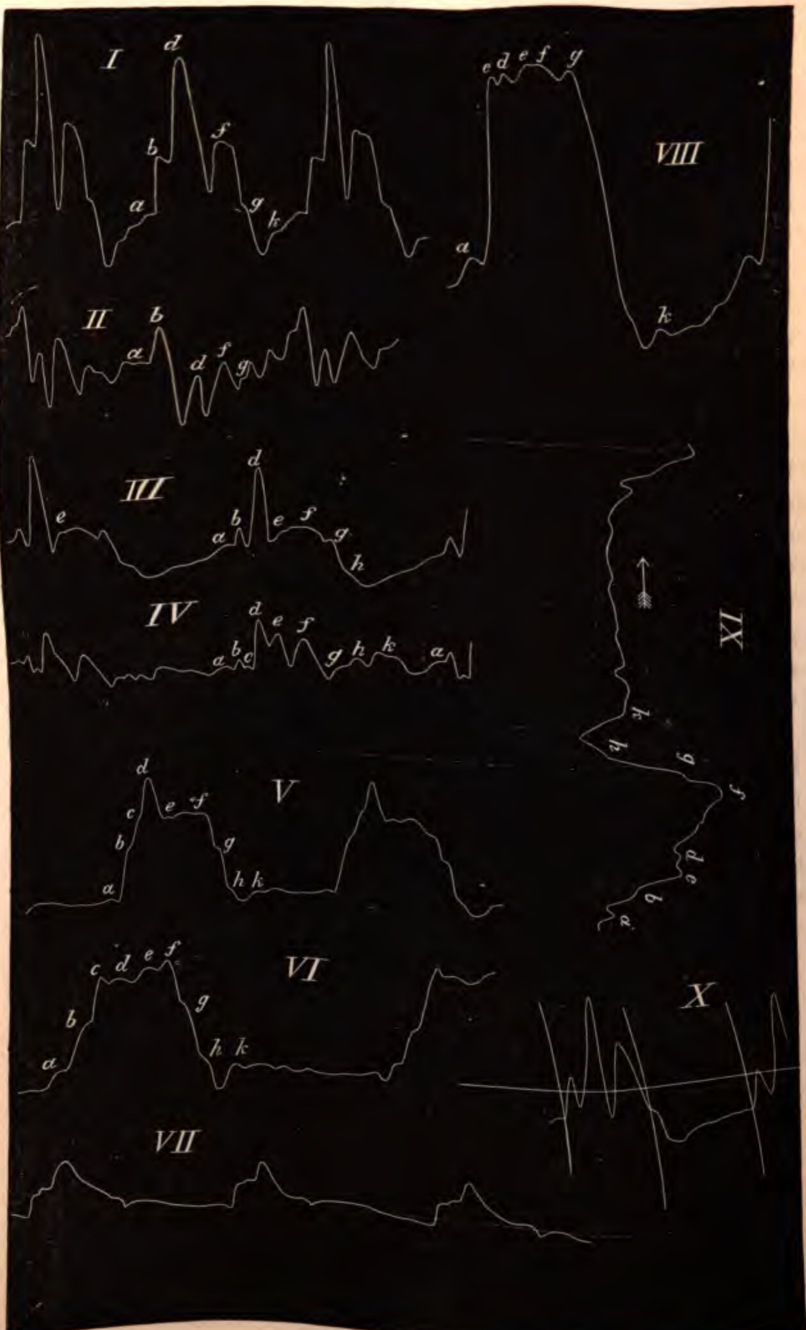


$xy = k\sqrt{x}$.
($k=22$)
($k=20$)
5·5.
5.
4·5.
4.
3·5.
3.
2·5.
2.
1·5.
1.
0.
10.
20.
30.
40.
50
60.
70.
80.
90.
100.
110.
120.
130.
140.
150.

the ratio to the whole is 1 : 3·3965, which is very near the former result. In slower pulses it is not easy to find an origin for the rise *f*, but if, as must be the case when the pulse is at 40 (its limit of slowness), the arterial tension is at its maximum, the rate of closure of the aortic valve must be at its maximum also; and the whole first part almost entirely occupied by the true systolé. When such is the case the equation $xy = k\sqrt{x}$ with $k = 22$ is satisfied by y being equal to 3·46 (about), which is curiously near the relation found in quick pulses, and tends strongly to shew, though these are all the grounds for it, that the length of the systolé of the heart is always a definite part ($\frac{10}{34}$th) of the whole pulsation, whatever its rate.

The traces from which the preceding observations have been made were all taken on myself, and the repetition of them by others at different rates of pulse would be a means of verifying or a cause for rejecting the results arrived at. The chief sources of error in finding the ratios given above, lie in the watchwork, which if not going at an exactly similar rate each time it runs, gives the rapidity of the heart incorrectly. Also, on starting, its speed augments for a short time and then decreases, both which cause variations from the true results.

By taking a trace after having remained some time in the hot room of a Turkish bath very rapid pulses can be recorded, up to and above 150 a minute in health, without the least inconvenience. Very slow pulses can be produced by lying nude some time in a cold air, or by drinking iced water, especially when nude.


I
II
III
IV
V
VI
VII
VIII
X
XI

Description of the Figures.

They were all taken one half the size here represented. Except Fig. IX. they read from left to right.

Fig. I. Apex trace of a heart beating 125 times a minute.

Fig. II. Trace of a heart beating 125 a minute, $\frac{1}{2}$ an inch internal to the apex beat.

Fig. III. Apex trace of a heart beating 88 a minute.

Fig. IV. Trace between 6th and 7th ribs, below the apex, of a heart beating 88 a minute.

Fig. V. Apex trace of a heart beating 76 a minute.

Fig. VI. Apex trace of a heart beating 50 a minute.

Fig. VII. Apex trace of a pulse of 90 a minute, taken with a lever 2 inches long, attached to the sphygmograph pad by a thread.

Fig. VIII. Apex trace of a pulse at 60 a minute, taken when the body was inclined forward.

Fig. IX. Apex trace of a pulse at 42 a minute. It reads from right to left.

Fig. X. Apex trace of a pulse at 103 a minute, prepared for measuring.

THE VARIETIES OF THE STYLOID MUSCLES. By

ALEXANDER MACALISTER, *Professor of Zoology, and Director of the Museum, University of Dublin.*

SOME time ago Mr Lawson Tait of Wakefield did me the honour of sending me notes of several interesting muscular anomalies observed by him in the neck of a male subject, which notes were published in the last number of the Journal. A press of unavoidable engagements prevented me at the time from examining my notes of the bibliography of the subject with any degree of care; but I have employed a few leisure moments since in collecting the described irregularities of one group of muscles specially concerned in Mr Tait's paper, viz. those arising from the styloid process; and I purpose, in the present notice, to tabulate as many of these as have come under my observation.

Of the three styloid muscles the stylo-hyoid is that most prone to vary, the stylo-glossus next, and the stylo-pharyngeus is the least liable to anomaly.

The stylo-hyoid is subject to the following varieties. Absence of it is described by Hallett and Böhmer (*Obs. Anatom. Rariorum*, Præf. Fascic. Alter. Halæ, Magdeburgh, 1702, p. v.). The former author describes its absence as occurring once in two hundred cases. Between the tendon of origin of this muscle and the styloid process, Sömmerring and Cloquet describe a bursa as existing (*De Corp. Humani Fabrica*, Tom. III. p. 118), but this I have not found to exist with a sufficient degree of frequency to lead me to regard it as a normal arrangement. A double stylo-hyoid muscle in which the two parallel muscles are nearly similar in attachments has been seen by Eustachius (Tab. XLI. fig. 5), Bidloo (Tab. XIV.), Santorini (*Observationes Anatom.*, Venet. 1724, p. 117), Wistar (*Anatomy*, Pennsylvania, 1823, p. 189), and Albinus; in these cases the relation of the two parts to the digastric tendon is not specially referred to, but cases in which the normal stylo-hyoid was completely split for its entire length, for the transmission of the digastric tendon, have been seen by McWhinnie and myself. I have like-

wise seen it undivided and passing behind the digastric tendon as described by Hallett. Much rarer, I think, is the form described by Sömmerring, which I have also met with, in which the entire muscle passes in front of the digastric. A singular form of the muscle in which the two slips were inserted, one into the front and the other into the back of the digastric tendon, has been seen, and is described by Haller (*Elem. Physiol.* III. 416), McWhinnie, and myself.

Many forms of second stylo-hyoidei are on record: one from the tip of the styloid process is described by Courcelles and Boyer (*Tr. Complet d'Anat.*, Paris, 1815, p. 87); one from the base of the process by Tait, as above referred to. Another form of this stylo-hyoideus alter is in the form of "an elegant small muscle" (Drake, *Anthropographia*, Book III. ch. 17) replacing the stylo-hyoid ligament; instances of this have been seen by Weitbrecht (*Comment. Petropolit.*, IX. 256), Blandin (*Nouveau Elém. d'Anat. Descriptive*, 1838, p. 374), and by myself. A somewhat similar instance is described by Gavarde as the petit stylo-hyoidien, passing from the tip of the styloid process to the lesser cornu of the hyoid bone, but the author does not mention whether it accompanied or replaced the stylo-hyoid ligament (*Traité de Myologie suivant la Méthode de Desault*, Paris, An. VII.). Santorini likewise mentions having seen a stylo-hyoid with a normal origin inserted into the apex of the lesser cornu of the hyoid bone (*Obs. Anat.*, Venetiis, 1724, p. 117). A stylo-hyoideus alter inserted into the cartilago triticea is described by Petsche (Haller's *Disput. Anatom. Select.*, VI. 767, and 1745, p. 107).

A second head for the normal stylo-hyoid from the angle of the jaw I have once met with, and my friend Mr Kelly, formerly Demonstrator of Anatomy at the Royal College of Surgeons, has communicated to me an instance of this kind. I have also seen this slip nearly detached from the rest of the stylo-hyoid. A still more curious slip I found (Nov. 1868) arising along with the stylo-hyoid and inserted into the angle of the jaw, forming a perfect stylo-maxillary muscle. A similar case is described by Mayer (III. 547). This muscle lay on the stylo-maxillary ligament. Slips of union from the omo-hyoid to the stylo-hyoid have been recorded as existing by Sömmerring

(p. 113), and the same author describes a slip from the stylo-hyoid ligament to the genio-hyo-glossus (p. 123). Another curious case of complexity is described by Mr Wood, in which a separate styloid fascicle of muscular fibres joined the posterior belly of the digastric, and this co existed with the insertion of the stylo-hyoid into the digastric tendon. A case of triplicity has been seen by Prof. Wenzel Gruber of St Petersburg; but, as far as I am aware, no other case has as yet been seen. The last variety of importance with which I am acquainted is the existence of a slip from the posterior border of the muscle passing under the hyo-glossus into the tongue.

The stylo-glossus normally occupies the lowest place in point of origin from the styloid process; and Cloquet states that it chiefly arises from the stylo-maxillary ligament (p. 285). Harrison describes it as arising from the inner side of the process near its point (*Dublin Dissector*, p. 38). Gray states that it arises from its anterior and outer side (p. 222). To this origin a second head is not unfrequently superadded from the stylo-maxillary ligament separate from the styloid head; 2nd from the inner surface of the internal pterygoid muscle (Moser, *Archiv für Anat.* VII. 226); 3rd as a fibrous slip from the angle of the lower maxillary bone (Riolan, p. 74, Winslow, p. 519); or 4th from the angle of the jaw as a fleshy band (Douglas, Theile). Of these forms I have seen the first and fourth. This second head may send some fibres into the stylo-hyoid; or, as before described, may unite solely with that muscle, but this is very rare; occasionally it may pass separately into the tongue, forming a mylo-glossus, as described by Hallett and Wood. A double stylo-glossus consisting of two parallel bellies has been described as occurring on both sides by Courcelles, Cheselden, Haller, Meckel, and McWhinnie, and in the form of two laminæ one superposed on the other, it is described by Fallopius (*Institut.*, p. 56). It has been seen sending deep fibres to interlace with the genio-glossus (Portal, *Cours d'Anat-Médicale*). Sandifort describes it as having been found cleft in one case, one slip passing to its normal insertion, and one into the pharynx (*Exercitationes Anat.*, Lugd. Batav. 1753, c. 6, p. 81). Its absence on one side was noticed by Albinus; on both sides by Böhmer (*loc. cit.* p. 6). In this case the other styloid muscles

were larger than usual; but in a case of the kind seen by Quain the stylo-hyoid and pharyngeus were of normal size. I have not included in the list of anomalies of this muscle the muscle of Duverney or the stylo-auricularis of Hyrtl; for a complete description of which see *Henle u. Pfeufer's Zeitschrift*, 3, Reihe x, Heft 3, p. 368, as this should not be enumerated as a part of the stylo-glossus.

The stylo-pharyngeus has been found double by Mr Wood as well as by Böhmer (*Institut. Osteol.*, p. 117, and *Obs. Anat. Rar.*, Præf. p. 26). In these cases both slips have the same insertion. In one instance Fallopius found a separate slip of this muscle inserted along with the stylo-glossus into the tongue. The same author has seen the entire of this stylo-pharyngeus passing to be inserted into the os-hyoides (*loc. cit.*, p. 75), or a slip of a normal stylo-pharyngeus may pass to the hyoid bone. Valsalva (*Epist. Anatom.* II. 4) and Morgagni (*Adversar. Anat.* I. 4) have described cases of this, and Santorini has found a slip passing from the stylo-pharyngeus into the thyroid cartilage (*op. cit.* p. 125). A few fibres have been traced by Walther into the epiglottis in one instance (Haller's *Disp. Anat. Select.*, Götting., Vol. I.). A slip has been traced to the glottis by Girardi (*Oratio de re Anatomicâ*, Parmæ, 1781, p. 36, note 1). When the fascicle to the thyroid cartilage is separate it is called by Luschka the stylo-laryngeus.

A remarkable triple stylo-pharyngeus is described by Haase (*Myotomiæ Specimen de Musculis Pharyngis*, Lipsiæ, 1784, p. 13) in which slips were traced, one to the uvula, one to the epiglottis, and one to the thyroid cartilage: another triple condition is described by Blandin (*loc. cit.* p. 383), one portion of which, coming from the spine of the sphenoid, is a true cephalo-pharyngeus, the second passes between the middle and the inferior constrictors of the phrarynx, and the third separates the middle from the superior constrictor. An accessory head has been described for this muscle by Prof. Luschka of Tübingen, passing from the mastoid process between the kerato and chondro-pharyngeus to join the rest of the normally arising stylo-pharyngeus.

ON THE VARIETIES OF THE PRONATOR QUADRATUS. By ALEXANDER MACALISTER, *Professor of Zoology in the University of Dublin.*

THE *pronator quadratus*, like many other muscles which are very definite in course and action, is singularly free from variation, and, as far as I am aware, there are very few anomalies of it described in anatomical works: Otto and Meckel describe cases in which it was suppressed. Blancard (*Anatomia Reformata*, Lugd. Batav. 1695) figures it as triangular with an apical tendinous, radial insertion, and Riverius (*De Dissectione partium Corporis Humanis*, Paris, 1545, p. 316) also represents it of this shape. Mr J. R. Barton, of the Philadelphia Hospital, has described a variety in which the muscle was divided into two triangular pieces whose apices were placed in opposite directions (Horner's *Special Anatomy*, I. 426).

In the Dublin Royal College of Surgeons' dissecting room I have met with several varieties of this muscle, some of which are not to my knowledge on record; and sketches of these I exhibited at the last meeting of the British Association at Liverpool (September, 1870). They were the following:

1. A triangular condition of the muscle similar to that described by Riverius and Blancard as above described; this form I have found with, or more commonly without a tendinous radial insertion, the radial attachment in two out of three cases was fleshy and not tendinous. In all the cases in which I have seen this triangular form, the apex has been radial and the base ulnar.

2. A bi-laminar condition of the ordinary muscle; this, I think, is a rare condition, but is referred to by Meckel, and in the only case in which it occurred simply the superficial lamina had a more extensive ulnar than radial attachment, and consequently its fibres sloped downwards and outwards, while those of the deeper layer passed directly across, or sloped gently in the opposite direction.

3. A bi-triangular form similar to that described by Dr Barton, as above quoted; in this instance the upper division had

an ulnar base and a radial apex, and the inferior had a radial base and an ulnar apex.

4. A trifid variety, in which the upper slip was triangular, with its base at the radius and its apex at the ulna; the middle portion was on a deeper plane and more nearly transverse in the direction of its fibres, while the inferior part was also triangular, with an ulnar base and a radial apex. Two specimens of this variety have fallen under my notice.

5. A trifid variety, in which the upper triangle completely overlay the middle deeper part, and had to be removed to exhibit it; the inferior triangle was as in No. 4.

6. A normal pronator quadratus, having below it a short small separate slip inserted into the sacciform capsule of the inferior radio-ulnar joint, and by a few fibres into the anterior ligament of the wrist-joint.

7. A triple condition in which the upper part had a radial base and an ulnar apex; the middle was, as in No. 5, deeper in plane and nearly transverse in direction; and the inferior, a small separate slip, arose from a little less than the lower half-inch of the anterior surface of the ulna, and was inserted by a narrow slip of tendon into the deep portion of the anterior annular ligament at its trapezial attachment. This variety resembled No. 4, but the insertion of the tendon of its inferior part extended towards the carpus.

8. The most remarkable of all the varieties met with was one in which the entire muscle, arising from the ulna as usual, passed outwards and downwards; all the fibres converging to a tendon which commenced near the centre of the muscle. This tendon crosses the anterior margin of the carpal end of the radius, and the anterior ligament of the wrist-joint, and ends by sending slips into the scaphoid, the trapezium, and the anterior annular ligament; a few scattered fibres extending as far downwards as the metacarpal bone of the thumb. This curious specimen I have presented to the Museum of the Royal College of Surgeons in Dublin, where it is preserved.

9. A case of a simple bi-laminar muscle, in which the deeper layer was attached, not to the surfaces, but merely to the interosseous margins of the ulna and radius, and thus pos-

sessed very short fibres; the superficial lamina was perfectly normal.

I have found these varieties commoner in the female than in the male, and in the left than in the right arm. I have never found them symmetrical; of the entire series Nos. 6, 7, and 8 are the most interesting, as showing the gradations of this muscle from its normal human forearm condition to the state of a muscle acting on the carpus. In No. 8, it presents the appearance of being a perfectly symmetrical antithesis of the *extensor ossis metacarpi pollicis,* and reminds us forcibly of the rare anomalous internal *peroneo-calcanian* muscle which I have elsewhere described.

CONTRIBUTIONS TO THE CELL-MIGRATION THEORY.

By RICHARD CATON, M.D., (Edin.), *Honorary Assistant Physician to the Liverpool Infirmary for Children.* Plate IV.

(Read at the Meeting of the British Association in Liverpool, Sept. 1870).

THE important facts in the physiology of the Blood Corpuscles, bearing on their migration from the blood-vessels, and their relations to the corpuscles of inflammatory lymph, pus or mucus, which, though investigated by Dr Wm. Addison in 1842, and demonstrated by Dr Augustus Waller in 1846, remained unconfirmed and almost forgotten, until their re-discovery by Prof. Cohnheim in 1867, have at length become so far familiar, that it is scarcely needful, in bringing forward further researches, to dwell on their early history at any great length.

It may however be well briefly to call to mind one or two of the leading points in the memoir by Professor Cohnheim, which anew directed attention to the subject, and formed a fresh starting-point for investigators[1].

The study of suppuration in the cornea of the frog first led this author to the opinion, that the pus-cells were not formed in the inflamed part, but migrated thither from the vessels.

In order to test this theory, observations were made on the mesentery of the frog; and here, when inflammation had been excited by access of air, or by the application of chemical irritants, the white corpuscles could be seen accumulating in great numbers in the veins, and ultimately migrating into the tissues. The process was found to commence in from four to fifteen hours after the exposure of the mesentery, and the time occupied by the escape of each cell was, on the average, about two hours, though liable to great variation. Migration of the red corpuscles was also observed.

The author supposed that the red corpuscles were forced out by the pressure of a congested state of the vessels, and that the colourless ones pierced the walls by means of the amœ-

[1] A list of papers referred to is given at the end.

boid movements with which they are endowed. From these observations he drew the important conclusion, that pus-cells are in all cases migrated white blood-corpuscles.

The great importance of these views naturally induced several physiologists and pathologists to repeat Cohnheim's experiments. Prof. Stricker of Vienna, Dr Kremiansky of St Petersburgh, Dr Charlton Bastian of London, M. Hayem of Paris, Dr Woodward of New York, and others, have made careful series of observations on the inflamed mesentery of the frog, with the result of confirming the description of cell-migration given by Cohnheim. On the other hand, Prof. Balogh of Pesth, and Dr Felts of Strasburg, have failed to see the migration.

In this paper, I shall first briefly give the results of a considerable number of original experiments on the mesentery of the frog, conducted in the mode described by Prof. Cohnheim, the frogs having been always previously paralyzed by the subcutaneous injection of woorali.

In those experiments which can be considered to have been successful, viz. in which the frog survived for a sufficient length of time after the exposure of the mesentery, the phenomena noticed were—1st: a slight dilatation of the vessels. The mesentery became perceptibly redder to the naked eye, than at the moment of exposure. For some time, quickening of the circulation was generally noticed. 2nd: a gradual retardation of the blood-flow, commencing in the veins and capillaries; movement continuing longest in the arteries; and when complete stasis had occurred in the veins and capillaries, advance and regress of the corpuscles in the arteries, corresponding with every systole and diastole, have sometimes continued for two or three hours: considerable congestion always took place. 3rd: when first observed after the operation, the white cells were being carried along in the general stream; in the veins generally occupying the "lymph-space," where they could be traced rolling along at a somewhat slower pace than that of the red globules. After a time, the white corpuscles became less regularly circular in outline, and shewed a tendency to

adhere to the wall of the veins; later still, large numbers of the white cells were deposited from the stream and covered the surface of the veins, and partially so of the capillaries. When the arterial current became slow, a few were also seen deposited in the arteries. The spontaneous or amœboid movements were now exhibited with considerable activity.

These appearances were seen in every frog which survived for a sufficient length of time after the exposure of the mesentery, and, in the majority of instances, nothing further occurred.

When commencing these observations, I operated on nearly a dozen frogs in succession, without seeing anything further than has now been described, notwithstanding that a most careful watch was kept on the mesentery, until the death of the frog, viz. from four to twelve or fourteen hours.

This failure I can only attribute, partly to defects in the method of operating, during at any rate the first few observations, such as the use of too large a dose of woorali; but chiefly to the fact, that nearly all these experiments were made in winter on hybernating frogs. During this time, the frog does not feed, even if hybernation be interrupted, and has but little activity and vigour.

After this long succession of laborious and unsuccessful experiments, when operating late in the spring, on strong healthy frogs, the migration was at length seen. In place of giving any general description, I will copy the notes of a single experiment, in the course of which the migration was very distinctly seen.

A strong active frog weighing between seven and eight drachms, captured on the day previous, being chosen; two and a half minims of woorali solution (one grain to the ounce) were injected under the skin of the back with a morphia syringe, at 10.45 a.m.: at 12, pulse 48; frog becoming paralysed: 12.50, p.m. pulse 44, paralysis complete: 1 p.m. peritoneum opened; no hæmorrhage from the incision; a part of the large gut withdrawn: 1.30, stasis occurring in a few capillaries: in the remaining vessels the current is so rapid, that individual cells cannot be distinguished. 1.40, pulse 80; arteries dilated: 6 p.m., pulse 56, current in the vessels is now much slower; a large

accumulation of colourless cells has taken place in the "lymph-space" of the veins. 6.45, the effects of the drug are seen to be subsiding: two minims more injected. 10.30, pulse 54; stasis has occurred in most of the capillaries; an active current still flows through the veins and arteries; the migration of a few white cells is seen to be commencing. 11.20, stasis occurring in all the vessels. 1 a.m., frog dead.

At the upper part of the plate accompanying this paper, will be found a series of sketches of the stages of migration of one of the cells watched during this experiment. I select this particular cell, because, as sometimes happens, the whole was visible during its transit (*i.e.* the part within the wall, as well as that protruding externally); this cell also contained two distinct granules, which made its identification more than usually easy. At 10.30 p.m., I first noticed a process from the corpuscle, apparently protruding into the substance of the wall of the vein. Ten minutes later, this process was seen to project slightly beyond the external margin of the wall; at 11 p.m., the projecting part had increased, and one of the two granules was seen to be, for the first time, lying external to the wall. A quarter of an hour later, both granules were in sight externally. The later stages were slower than the earlier: at 11.40 the cell had only a slight attachment to the wall, and at 12.20 a.m. appeared wholly free.

I have never yet been able to see the migration of large numbers of colourless cells in progress at the same time, nor yet the migration of the red corpuscles. With these exceptions, which may be accounted for by the comparatively small number of observations made, the appearances seen in the inflamed mesentery of the frog, have been in accordance with Cohnheim's description.

In addition to the investigations made on the mesentery of the frog, by the authors above named, several of them have also examined the transparent membranes of other animals, with the same object. Dr Kremiansky studied inflammation in the wing of the bat, and in the mesenteries of the dog and rabbit. The mesentery of the mouse, the tongue of the frog and of the

toad, have also been examined in search of "auswanderung;" but though the process has been traced in all these, it has been with less distinctness, than in the mesentery of the frog. Two of the most important vascular membranes had not been explored, viz. the tail of the fish, and that of the tadpole. The study of these was doubtless prevented by the special difficulties which attend the microscopic examination of animals breathing by gills. Having contrived a simple stage-trough (described in the July No. of the *Quarterly Journal of Microscopic Science,*) which enabled the fish or tadpole to be studied with great ease, I began a series of observations on each, and shall now briefly give the results[1].

The Fish.

The earlier stages of the inflammatory process in the fish, on the whole, resemble those in the frog. There is greater activity in the amœboid movement of the colourless cells, and these adhere to the walls of the veins in the same manner. One peculiarity is worthy of mention, viz. that congestion is not seen to occur to the same extent, as in batrachians and mammals. The vessels, especially the veins, are not seen so much engorged with corpuscles. It seems not improbable that the reason of this is to be found in the peculiarity of the heart. The motive power is placed further back in the circulation; all the suction of the auricle and the propelling force of the ventricle are exerted on the venous side of the branchiæ, instead of being partly on the one side and partly on the other. The effect of this arrangement may be that congestion takes place in the branchial rather than in the systemic circulation. This point has some little interest, in connection with the rationale of migration, to be considered presently.

During a long series of experiments on inflammation in the fish, a process like suppuration in higher animals, was observed. Cells resembling white corpuscles were formed in the stroma, or, after removal of the epithelium, on the surface of the membrane; and, when inflammation went on to a destructive ex-

[1] Since making these experiments, I have learned that Prof. Stricker and other Vienna pathologists, have recently studied migration in the tadpole.

tent, the tissues were seen to break down into a cellular débris resembling pus. Notwithstanding close and long-continued observation, "auswanderung" was never seen to take place. The walls of the vessels are more transparent than those of the frog, a clearer view is obtained, and observation is altogether easier, so that had migration occurred, it would probably have been seen.

The frog is apt to die before inflammation is sufficiently far advanced, and thus failure may easily occur; but in the fish, the inflammation produced is not fatal. In many instances I have traced it for two or three days consecutively in the same specimen.

One of the most noteworthy facts observed, was the appearance, during inflammation, of a system of delicate lines having a distinct double contour, running usually at right angles to the course of the large vessels. They are quite invisible in the healthy condition; possibly they may be minute lymphatics, or a "saftkanalsystem," in connection with the blood-vessels. I have succeeded in mounting and preserving specimens which, after the lapse of six or eight months, still show these lines with tolerable distinctness.

The study of inflammation in the fish is very interesting. As however cell-migration apparently does not occur, I shall not, in this paper, enter into any further description.

THE TADPOLE.

Migration of blood-corpuscles, red and white, may probably be said to have its maximum activity in the tadpole. The process takes place to so great an extent, that, when commencing observations on the tadpole in April last, I was almost in doubt, whether or not migration might possibly be a physiological occurrence, and be connected in some way with the nutrition of the tissues.

Kölliker appears to have been the first to observe the escape of corpuscles from the vessels of the tadpole, though his interpretation of it was apparently mistaken. His remarks are thus summarised by Todd and Bowman (*Physiol. Anat.* p. 277): "He found that when the current of the blood was regular,

there was no appearance of communication between the two orders of vessels (lymphatics and blood-vessels); but that when the circulation was excited and tumultuous, owing to the confinement of the tadpole under glass, blood-corpuscles escaped more or less readily from the blood-vessels into the contiguous lymphatics; and in several instances he was able to detect actual communications between lymphatics of the finest kind, and the network of capillary blood-vessels." "After careful enquiry, however, he concludes that these junctions are due to rupture, or perhaps in some cases to primitive abnormal formation."

In a congested state of the circulation, corpuscles rapidly escape from the vessels at any part of their course. Occasionally one is seen, passing out exactly at the point where one of the large stellate corpuscles, figured by Kölliker, chances to approach the vessel; but so large a number are seen escaping at points where there are no such stellate corpuscles, that the former conjunction is evidently a mere chance, and does not indicate the passage of a cell from the blood into the lymph-channels.

It is less easy to excite inflammation in the tadpole than in the frog or fish. The vessels have indistinctly defined walls, and appear as though they were little more than channels hollowed out in the jelly-like substance of the tail: the delicacy of organization is so great, that extensive rupture of the vessels, and extravasation of blood, is apt to follow injury to the tail. The animal, moreover, can only be kept under the microscope for about three hours at a time, and seldom survives a second examination. The whole course of the inflammatory process cannot therefore be studied so well as in the frog or fish. Fortunately this is not needful for the purpose of observing the migration. The occurrence of the latter process is liable to great irregularity; in some cases considerable irritation fails to bring it on, in others it takes place spontaneously and very early after the animal has been placed on the stage.

Any interference with the return of venous blood, produces migration of red corpuscles very quickly, just in the same manner as Cohnheim has shown to occur in the foot of the frog when the femoral vein is tied. If any injury, or the irritation produced by a constrained position, has produced that peculiar

softened and plastic condition of the white cells in which they adhere to the walls of the veins, and if a coating of white cells has in consequence lined the veins before congestion occurs, the white cells migrate in place of the red. It is not uncommon to see, at the same time, white cells escaping from a vein, and red ones from another vessel adjoining.

The sketch occupying the lower part of the plate, was drawn from a tadpole in which "auswanderung" of both forms of corpuscles, was in active progress. The animal—which was rather weakly, from having been kept for two months in a tank—was put into the stage-trough at 4 P.M. That portion of the tail represented in the sketch, contains a medium-sized forked vein crossed, at its upper part, by a large capillary which arose from an artery near at hand. When first seen, the circulation was proceeding naturally in both vessels, being more rapid in the capillary; white cells were seen rolling along in the usual manner in the lymph-space of the vein. External to the vessels, were seen the ordinary stellate corpuscles of the tadpole. Nothing resembling a blood-cell was to be seen in the parenchyma.

At 4.30 circulation was becoming more rapid, and colourless corpuscles were beginning to adhere to the wall of the vein. At 5 the vein was becoming closely lined with white cells. At 6 p.m. congestion had occurred in both vessels somewhat suddenly. A slow current still traversed the vein, but stasis had occurred in the capillary. Active migration of large numbers of white corpuscles was seen to take place from the vein. The capillary had become so much congested, that few cell outlines were to be seen in it. Numerous yellow button-shaped and irregular prominences were observed forming on its sides and upper surface. One or two small extravasations occurred, as seen on the right-hand side of the drawing. The mode of formation of these extravasations is very peculiar. Essentially they are the same as the other smaller prominences, and commence in the same way. A little button-shaped body, as I have said, is seen to form on the surface of the vessel; this increases, becoming globular. Secondary prominences form on the surface of this; they in turn enlarge, and give rise to similar off-shoots, until a mass is formed, like that seen in the drawing. The red cor-

puscles are soft jelly-like bodies, and readily take any form impressed upon them. When escaping from the vessels, they become more or less spherical, probably in consequence of the resistance of the tissues into which they have penetrated. These extravasations are sometimes seen to extend with great rapidity, being evidently fed from the original aperture opposite which the first little prominence was seen.

The white corpuscle appears in some cases to pass out through a very small aperture, to judge by the diminutive size of the first protruding part, and the narrowness of the neck which, a little later, is seen to join the part within to the part without the vessel. In other cases it has seemed as though the cell sank through the wall, without undergoing much change of form, in the manner which Dr Bastian has stated to occur sometimes in the frog.

The migration of colourless cells takes place more rapidly in tadpoles which are debilitated and affected with parasites from having been long in a tank and kept on artificial diet. While examining specimens in this condition, I have observed one really interesting and important fact, viz. *the apparent occurrence of migration of red and white cells, in the absence of local inflammation.*

A number of tadpoles of the frog and toad, which have been in captivity nearly three months, are found to exhibit this interesting phenomenon almost every time they are examined. However carefully everything approaching to injury or irritation of the tail is guarded against while the tadpole is being placed in the trough, in less than an hour, as a rule, both red and white cells begin to migrate.

After being for a little time in the stage-trough, the restraint consequent on being laid on its side, with a cover-glass resting on the tail, appears to produce a slightly feverish condition of the tadpole, the heart acts more quickly, and by degrees the white cells are deposited in the veins. Respiration can of course scarcely be expected to be quite so easy when the animal is in the trough, as when free; and this is especially the case in a tadpole of the age named, when the lungs are becoming developed and a frequent supply of air is needed. Probably from this cause congestion soon occurs, and

migration is seen to take place actively when there is no appearance of any local irritation.

Only one point remains to be mentioned, viz. that two or three times during inflammation, bodies indistinguishable from white blood-cells have been seen situated about the centre of a stellate corpuscle, at a long distance from any vessel. They had probably been formed in that position; the process of their formation I have not yet been able to trace.

The above are some of the chief facts of cell-migration. One or two questions of theory now remain to be considered:

I. The mechanism of Cell-Migration.

II. The relation of the process to Suppuration.

I. Professor Cohnheim supposes that the red corpuscles are forced out of the vessels by the pressure produced by congestion, and that the white corpuscles make their way through the wall by virtue of their amœboid movements. It may be objected to this theory, that if congestion will account for the escape of the one, it ought to do so also for the other.

It seems very probable that congestion is the cause of the migration of the red corpuscle. The occurrence of migration in the frog's foot after ligature of the femoral vein, also in the tadpole when venous return is impeded, likewise its non-occurrence in the fish, where congestion scarcely occurs, all point to this conclusion. I shall try to shew that the same cause operates in the case of the white cells.

It is a fact at present unexplained, that the white corpuscle is attracted to the walls of vessels more than the red. This affinity is chiefly exhibited in the veins, where the slower current permits the colourless corpuscle to roll tardily along in the lymph-space. When any general fever, or tendency to local inflammation exists, this attraction is increased, and the white cells are rapidly deposited, forming often a complete coating to the veins. When such a vessel becomes gorged, and the tension is so great that its contents are forced out through the walls, it follows, as a matter of course, that the corpuscles nearest the wall pass out first, and these are the white cells.

On referring to the drawing taken from the tadpole, it will be evident that if the cells are to be forced out through the walls singly, white cells will escape from the vein and red from the capillary. If the engorgement be produced artificially in the tadpole, before any deposit of white cells has taken place, escape of red corpuscles occurs from veins, arteries and capillaries alike. In higher animals it is well known that congestion produces œdema, and it may probably do so in the frog and tadpole, though of course the fluid cannot be seen passing out under the microscope; but, supposing currents to pass out from the vessels through the apparent orifices described by Recklinghausen, it is reasonable to believe that corpuscles lying adjacent to these apertures might be forced out at the same time.

With regard to the likelihood of the cells piercing the wall by virtue of their amœboid movements, it may be remarked, that that property appears to be specially active in the fish, but that nevertheless migration seems not to occur. On the whole, it seems probable that the occurrence of migration depends on two factors, viz. (1), The degree of pressure in the vessels produced by congestion, and (2), The power of resistance possessed by the walls of the vessels. Thus it is seen, that in the fish the absence of any considerable congestion prevents the occurrence of migration; while in the tadpole, the frequency of congestion, along with the extreme tenuity of the walls of the vessels, causes it to happen with remarkable facility.

Further, the question whether the red or the white cells shall escape, depends simply on the accident of the one or the other being in contact with the wall of the congested vessel.

II. The relation supposed to exist between "auswanderung" and suppuration has given the subject its chief interest: Professor Cohnheim maintains that all pus-cells are migrated white blood-corpuscles. This opinion has been shared by but few of those who have otherwise confirmed his observations. Dr Kremiansky of St Petersburg, who made a most extensive series of experiments, was led to believe that pus originates only in part in this manner; such also is Dr Bastian's view, and this opinion has also been supported by Messrs Paget and Turner[1].

[1] *Lectures on Surgical Pathology*, pp. 249, 250. Third Ed. 1870.

Regarding the argument drawn from experiments on the cornea, Hoffmann and Recklinghausen, after a special enquiry, still maintain the old opinion that the pus-cells are formed from the existing cell-elements; and more recently Professor Stricker and Dr Norris (now of New York) have again repeated the experiment on the cornea, and confirm the results of Hoffmann and Recklinghausen.

In the experiments described above, suppuration was observed in the fish without any migration being seen; in the tadpole migration was seen within an hour after the animal had been placed under the microscope, when all injury and local irritation had been carefully avoided. It seems scarcely possible that true suppuration could be produced by so slight a cause and in so brief a time. Such an opinion would be contrary to all experience of that process.

Thus I think it may be said, with great probability, that suppuration was seen apart from "auswanderung," and "auswanderung" apart from suppuration in these two cases respectively.

The question cannot yet be said to be settled. It is at least improbable that pus originates solely from migrating blood-cells; while, on the other hand, it is possible that there may be no connection whatever between them.

LIST OF PAPERS RELATING TO THE MIGRATION THEORY.

Addison. A series of articles in the *Medical Gazette*, "On the Physiology of the Blood." 1840—42. Especially 14 March, 1841. "Experimental Researches into the Phenomena and Products of Inflammation," in *Trans. Prov. Med. Assoc.*, 1st series, 1842; 2nd series, London, 1843; 3rd series, London, 1845.

Waller. "Microscopic Observations on the Perforation of the Capillaries by the Blood, and on the Origin of Mucous and Pus Globules." *Philosophical Magazine*, 1846.

Zimmermann. An article in *Medicin Zeitung des Vereins für Heilkund im Preussen.* 1852.

Cohnheim. "Ueber Entzündung und Eiterung." *Virchow's Arch.* Bd. XL. 1867; also Bd. XLI.

Hoffmann and Recklinghausen. "Ueber die Herkunft der Eiterkörperchen." *Centralblatt*, No. 31, 1867.

Kremiansky. "Experimentale Untersuchungen über die Entstehung und Umwandlung der histologischen Entzündungsproducte." *Wiener Medicinische Wochenschrift*, Nos. 1—6, 1868.

Koster. "Exudation of Colourless Blood-Cells," &c. *Journal of Anat. and Phys.* III. 246.

Bastian. Two papers read before the Pathological Society. *Path. Trans.* XIX. 466.

Balogh. "In Welchem Verhältnisse steht das Heraustreten der Farblosen Blutzellen," &c. *Virchow's Arch.* XLV. 19.

Schklarewski. "Prof. Balogh und die Auswanderung." *Virchow's Arch.* XLVI. 116.

Axel Key. Article on Professor Key's Pathological Researches. *Med. Times and Gazette*, May, 1869.

Norris and Stricker. Account of Experiments on the Cornea in Stricker's *Studien an den Inst. f. Exper. Path. in Wien.* 1869.

Felts. "Recherches Experimentales sur le passage des Leucocytes à travers les parois Vasculaires." *Robins' Journal de l'Anat. et de la Physiol.*, Jan. 1870.

Hayem. "Le Mécanisme de la Suppuration." Read before the French Academy of Medicine, 25 Jan. 1870, and noticed in *Archives Gen. de Médecine*, March 1870, p. 364 and 369.

Woodward. A Lecture delivered at the College of Physicians, Philadelphia, 31 May, 1869. Noticed in Dr Richardson's paper, *American Journal of Medical Science*, Jan. 1870.

Stricker. "Studies on Inflammation." *Quarterly Journ. Micr. Science.* July, 1870.

ON THE LOSS OF SOLID MATTER DURING THE GERMINATION AND EARLY GROWTH OF PLANTS. By ARTHUR RANSOME, M.D., M.A., *Cantab.*

THE following statement, with reference to the conditions of growth, is made by Prof. Draper in his work *On Human Physiology* (New York, 1856, p. 458): "If growth be conducted in darkness, heat, air and water cannot cause the young plant to add anything to its substance—it is feeding on the seed. Indeed, when the experiment is carefully made, it is found that there is an actual loss of substance; the resulting plant, if dried, weighing less than the dry seed from which it came. In a dark place, then, it is possible for a seed to grow, but it grows in a certain way and only to a certain extent. Its stem and its leaves are of a sickly yellowish hue; exposure to sunshine soon produces a green colour in these parts, and the weight of the plant increases. Growth in darkness leads to one result, and growth in the sunshine to another." This observation was not accompanied by any reference to experimental proof; and although it seemed very likely to be correct, it appeared desirable to ascertain its truth by direct investigation.

The following experiments were undertaken with this object: given weights of fresh mustard-seed were taken, and each portion was divided into two equal parts, one of these parts was then crushed and moistened to set free any volatile oil, and was afterwards carefully dried in a water-bath, or in Dr Calvert's silk-conditioning apparatus, which he kindly allowed to be used. The other portions were sowed upon well-washed old flannel, placed upon moistened saucers and watered, in three instances with distilled water, in four others with the Manchester town's water.

The experiments were carried on through the months of June, July and August of 1858.

In four instances the seeds were left to the full influence of diffused daylight, on the ledge of a window, looking west, in two others, the saucers were placed in a large, perfectly dark cupboard, and only watered at night by artificial light; and in one case the seeds were deposited in a dark wine-cellar. After growth the plants were carefully dried and weighed.

The following table gives the results of all the experiments:

TABLE 1. GROWTH OF MUSTARD.

No.	Conditions of growth.	No. of days of growth.	Loss or gain per cent. after drying.	Remarks.
1	None	None	−12	The seed was crushed and moistened, before drying.
2	In light. Town's water and air only.	8	−22·5	Plants ½ in. long, green leaves just emerging.
3	ditto	10	−29·8	Plants 1½ inch long, strong and healthy.
4	In light. Distilled water used.	14	−26·7	Small amount of growth about 1¼ inch.
5	ditto	17	−37·9	Stalk and leaves about 2 inches long, strong and healthy.
6	In darkness. Town's water and air only.	10	−27·8	Etiolated, but strong plants, about 2 inches in length.
7	ditto	13	−40	Plant very weak, about 2½ to 3 in. in length.
8	ditto	17	——	Length of plant about 4 in. dead and decomposed.

The results were somewhat remarkable and show that, at any rate, during the germination and early growth of mustard-seed, Prof. Draper's statement does not hold good.

1. In every case, whether in light or darkness, the plants (root, seed, stem and leaves) when dried, had lost a certain amount of solid matter.

2. Up to the period of cultivation observed the amount of loss was in close relation to the degree of growth.

3. Up to a certain stage of growth, there was but little difference in the extent of loss, in the light and in the dark.

In fact up to 10 days of growth, the greatest loss was in plants grown in the light.

These experiments were repeated with different soils with the bulbs of hyacinths, crocus and snowdrops, in the winter and spring of 1865—66; and in the following year kidney beans and peas were grown in garden mould, carefully washed and dried after growth, and then tested in the same way as the mustard-plants. It was found that the peas and beans only began to gain in weight when the plants were from 8 to 12 inches in height. The following tables II. III. and IV. give the results with the bulbs.

It may be seen from these Tables that, although the growth of the plants named was carried on for a considerable period, and under differing conditions of soil, light, &c., yet in every case the result was the same, whether water, sand, earth-mould or cocoa-fibre was used to bed the bulbs, and whether they were cultivated in the darkness or in the light. Several of the hyacinths were allowed not only to form buds, but to flower, and in two instances the flowers were allowed to wither before the plants were removed from the glasses and dried, one of the plants grown in the dark also had buds about to burst; and although the leaves, stalk and flowers were a pale yellow colour, it seemed to be otherwise quite healthy. The crocus also had formed flowers in the sheaths, and the snowdrops had produced buds; notwithstanding this, a loss of substance up to the period of growth observed was noted in every case, and to some extent it increased with the growth of the plant. In the darkness, in some instances the loss of weight was less than in the light, in others there was little or no difference.

In the case of bulbs it seems probable that it is only when the plant has ceased flowering, and when the secondary bulbs are being formed, that there is any material gain in weight.

In searching for a record of any similar investigations by others, I found that in the year 1838, Mons. Boussingault had made similar experiments upon the growth of wheat and trefoil, and although he did not trace the full extent to which these plants lost substance, before they began again to increase in weight; yet he had gone further in other respects, and had ascertained in what constituents of the plants the loss had

TABLE 2. GROWTH OF HYACINTHS.

No.	Weight of bulb in grains.	Date.	Weight of dried bulb and plant, &c. in grains.	Loss per cent.	Date.	No. of days' growth.	Conditions of growth.	Remarks.
1	888	1865 Oct. 27	330	62·7	1865 Oct. 29	None	None	Just germinating.
2	960	,,	380	60·4	,,	,,	,,	,,
3	609	,,	160	73·8	1866 Mar. 5	129	In dark cellar, water and air only.	Leaves 6 in. long. Flower stalk 10 in. long. Buds about to burst, whole plant light yellow.
4	835	,,	269	67·8	Jan. 31	96	In light, water and air only.	Plants 5 in. long, strong and healthy, flower-buds appearing.
5	671	,,	177	62·1	Feb. 26	122	ditto	Leaves 10 in. long, flowers withered and dry.
6	794	,,	187	76·5	Mar. 12	136	ditto	Two large spikes of flowers, 10—12 in. long, strong healthy flowers and leaves.
7	960	not noted	410	57·4	not noted	not noted	ditto	Green leaves 3 in. long.
8	890	,,	320	64·2	,,	,,	ditto	Green leaves 5 in. long, buds well formed.
9	1020	,,	240	76·5	,,	,,	ditto	Healthy plant, had completed flowering.

TABLE 3. GROWTH OF CROCUS.

No.	Weight of the fresh bulb in grains.	Date.	Weight of dried bulb and plant, &c. in grains.	Loss per cent.	Date.	No. of days' growth.	Conditions of growth.	Remarks.
1	150	1865 Oct. 20	88	42·4	1865 Oct. 20	None	None	Just sprouting.
2	116	,,	70	39·7	,,	,,	,,	,,
3	148	,,	82	44·6	1866 Jan. 22	94	In darkness. Earth and cocoa fibre.	4 inches long, plant yellow, otherwise healthy.
4	116	,,	57·5	50·5	1865 Dec. 20	61	In light. Earth and cocoa fibre.	4 inches long, slightly singed in drying.
5	111	,,	46·0	58·6	1866 Jan. 17	89	ditto	4 inches long, flowers just formed in sheath.
6	126	,,	62·5	50·4	Jan. 26	98	ditto	Diseased.
7	115	,,	53	54·7	Mar. 3	134	In diffused light in cellar, earth and cocoa fibre.	7 inches long.

TABLE 4. GROWTH OF SNOWDROPS.

No.	Weight of the fresh bulb in grains.	Date.	Weight of dried bulb and plant, &c. in grains.	Loss per cent.	Date.	No. of days' growth.	Conditions of growth.	Remarks.
1	48	1865 Oct. 24	22·5	53·2	1865 Oct. 24	None	None	No growth.
2	48	„	21	56·3	„	„	„	„
3	37	Oct. 20	17	60	Dec. 29	70	In light, in sand, in cellar.	About 2½ inches long, burnt in drying.
4	42·5	„	14·5	65·5	1866 Jan. 5	77	ditto	About 3 inches long, slightly burnt in drying.
5	42	„	17	59·6	Jan. 8	80	ditto	ditto.

occurred. M. Boussingault's researches were undertaken with the object of learning whether plants take nitrogen from the air, but they seem to have attracted little notice from physiologists. The results of his experiments are however in close accordance with those which have just been mentioned, and they show, in addition, in which of the elements the chief changes take place.

The following tables contain the results given by Mons. Boussingault in the *Comptus Rendus*, VI. 102.

1. In the early stages of germination and growth the following results were noted:

A. Germination of Clover.

1st period when the radicles are developed:

Grammes.	Carbon.	Hydrogen.	Oxygen.	Nitrogen.
2·893 of grain containing (2·405 dry ?)	1·222	0·144	0·866	0·173
gave				
2·241 of germinated grain (2·240 dry ?)	1·154	0·141	0·767	0·178
– ·165 differences	– 0·068	– 0·003	– 0·099	+ 0·005

2nd period when the seminal leaves were formed:

Grammes.	Carbon.	Hydrogen.	Oxygen.	Nitrogen.
2·074 of grain containing	1·054	0·124	0·747	0·149
gave after growth				
1·727 „	0·817	0·104	0·656	0·150
– ·347 differences	– 0·237	– 0·020	– 0·091	+ 0·001

B. Germination of Wheat.

1. To appearance of radicles:

Grammes.	Carbon.	Hydrogen.	Oxygen.	Nitrogen.
2·429 of grain containing	1·132	0·141	1·073	0·083
gave after germination				
2·401 (2·363 dry ?)	1·111	0·139	1·026	0·087
– ·066 differences	– 0·021	– 0·002	– 0·047	+ 0·004

2. When the young stem is the length of the grain:

Grammes.		Carbon.	Hydrogen.	Oxygen.	Nitrogen.
2·130 of grain	containing	0·993	0·124	0·940	0·073
gave					
2·096 (2·053 ?)	„	0·932	0·121	0·929	0·075
–·073 differences		– ·061	– ·003	– ·011	+ ·002

3. When the stem is from 3 to 5 centimetres high:

Grammes.		Carbon.	Hydrogen.	Oxygen.	Nitrogen.
2·075 of grain (2·027 ?)	containing	0·945	0·117	0·895	0·070
gave					
1·704 (1·703 ?)	„	0·804	0·104	0·723	0·072
–·324 differences		– ·141	– ·013	– ·172	+ ·002

Mons. Boussingault now apparently discontinued the analysis of plants until they had attained a growth of 2 or 3 months; his results were then as follows:

C. Cultivation of Clover in pure Silicious Sand and Distilled Water.

1. During 2 months (Sept. and Oct.).

Grammes.		Carbon.	Hydrogen.	Oxygen.	Nitrogen.
1·532 of grain	containing	0·778	0·092	0·552	0·110
gave					
1·649 of crop (2·526 ?)	„	1·278	0·146	0·982	0·120
·994 differences		+0·500	+ ·054	+ ·430	+ ·010

2. During 3 months (August, Sept. and Oct.).

Grammes.		Carbon.	Hydrogen.	Oxygen.	Nitrogen.
1·586 of grain	containing	0·806	0·095	0·571	0·114
gave					
4·106 of crop	„	2·082	0·271	1·597	0·156
2·520 differences		+1·276	+0·176	+1·026	+0·042

D. Cultivation of Wheat.

1. During 2 months (Sept. and Oct.).

Grammes.		Carbon.	Hydrogen.	Oxygen.	Nitrogen.
1·244 of wheat containing		0·580	0·072	0·549	0·043
gave					
1·819 of crop	„	0·901	0·116	0·762	0·040
+ ·575 differences		+ ·321	+ ·044	+ ·213	− ·003

2. During 3 months (August, Sept. and Oct.).

Grammes.		Carbon.	Hydrogen.	Oxygen.	Nitrogen.
1·644 of wheat containing		0·767	0·095	0·725	0·057
gave					
3·022 of crop	„	1·456	0·173	1·333	0·060
+ 1·378 differences		+ 0·689	+ 0·078	+ 0·608	+ 0·003

The tables as given in the *Comptus Rendus* are not perfectly clear; some errors seem to have crept into them, but, so far as they go, they support the conclusions to which my experiments lead, and they show moreover in which of the elements the chief loss or gain takes place. It will be seen that in the early stages of growth, there is little if any variation in the quantity of nitrogen contained in the plant, but what little alteration there is, is on the side of increase; the hydrogen also is only slightly altered by diminution, but the carbon and the oxygen both disappear rapidly, and they evidently disappear for some time in proportion to the growth.

In the later stages of the cultivation of both wheat and clover the carbon, hydrogen and oxygen again increase. In the clover the amount of nitrogen also increases, but in the absence of all manure the nitrogen of the wheat remains stationary.

These facts seem to have an interesting bearing upon the physiology of nutrition and development. They appear to show that a close analogy exists between the two kingdoms of nature, so far as regards the functions of early growth. Thus as the animal ovum contains nourishment to support the embryo for a certain period, and, under fitting conditions, the nutritive material gives forth force to stimulate and sustain the vital

powers of the minute germ, so apparently, the genesis of active life in the plant must receive its first impulse from the constituents of the seed, and in most plants at least a portion of the power of growth must be derived from the same source.

Again, in the animal body, at certain periods, material is stored up, from which supplies of force may be drawn as it is required, and thus the creature can live and grow for a certain period sustained by its own substance. In like manner, it seems from the experiments quoted, that bulbous and probably other plants derive a considerable part of their sustenance from food which has been laid up in the bulb or seed, collected there probably during an apparent rest from growth.

The results of the experiments may perhaps also afford an illustration of the distinctions between growth and development, and nutrition.

There cannot of course be growth or development of any part of a plant or animal, without the addition of substance to that part; and yet, as we have seen, both plants and animals may on the whole lessen in weight during growth. In the case of bulbs, the plants grow and flourish, sustained, for the most part, for very long periods upon the material previously stored up in the bulbs; and the same statement is true, though for a shorter period, of the smaller seeded plants.

We might perhaps with propriety limit the term nutrition, to the act of taking in and storing the food for future growth; and in this sense the nourishment of the hyacinth plant, as a whole, takes place only when it begins to take up fresh solid material from without.

It is important also to notice the source from which the power of growth appears to spring in the first instance.

The elements which are used up during early growth are the carbon and oxygen, and to a certain extent the hydrogen of the seed; and we know at least partially in what way these elements diappear.

During germination, the starchy portions of the seed, by a species of fermentation, under the influence of warmth and moisture, and also probably by the molecular action of the nitrogenous germ, become changed into sugar and other soluble substances, and these again are slowly disintegrated and burnt

up, by a kind of respiration, and heat or other energy is developed.

In any case it would appear that even in this early stage of the life of a plant, the most important agents in assisting the processes of growth, are hydrocarbonaceous particles in their course of transformation; and there is little doubt that the heat energy of these compounds bears some important relation to active life.

This fact may indeed be placed in close connection with the recent investigation of Messrs. Fick and Wislicenus, Frankland and Haughton, upon the sources of mechanical power in animals. It seems probable that the oxydation of carbonaceous compounds in animals has something to do, not merely with the phenomena of muscular contraction, but with many other vital processes.

It may be found that plants also owe not a little of their power of growth to the same source. It appears from the observations detailed in the tables, that the plants which they record lost weight during growth, almost equally in the light and in the darkness. It may be possible therefore that the processes of development and growth may obtain the needful stimulus, not only from the direct heat and light of the sun, but also from the energy lying hid in the carbonaceous compounds already stored up within the seed or bulb.

It would perhaps be interesting in reference to this point, to compare the conditions of early growth of different plants with the constituents of their seeds. It might be found that those plants which are able to grow for some time without the light, have the largest store of heat energy bound up within their substance.

After all, however, light is evidently essential to the true nutrition of plants. It may not be necessary, and these experiments show that it is not necessary, to some kinds of development and growth, but it is needed before any material addition can be made to the solid substance of vegetable structures. In this sense Prof. Draper's words probably still remain true, and "growth in darkness leads to one result, growth in the sunshine to another."

ON THE HOMOLOGICAL RELATIONS TO ONE ANOTHER OF THE MESIAL AND LATERAL FINS OF OSSEOUS FISHES. By PROFESSOR HUMPHRY. Pl. II.

THE use of the terms 'median' and 'lateral' with reference to the fins of fishes, though founded on obvious facts, appears to me nevertheless to have in some measure the effect of contributing to maintain a misconception with regard to the real nature of those parts and their relation to one another and to surrounding structures. More especially is this so forasmuch as the term 'median' is by anatomists pointedly contrasted with 'lateral' when they are indicating as one among the special features of fishes that certain of their locomotory organs are placed in the middle line.

The most rudimentary knowledge of development tells us that, with the exception of the parts lying in the axis of the body, none are really mesial, that is, formed in the middle line. All are formed in the lateral layers which ascend or descend from the axial line. The most, therefore, that we can say of any part is that it has acquired a mesial position and character by the coalescense of two lateral elements in the middle line. This must be the case with the dorsal, caudal and anal fins of fishes; and it is in this way that, though not essentially median organs, they have acquired their claim to that title. Nor is this an unimportant distinction, especially when we seek to compare them morphologically with other structures. Each of these fins, like the tongue and every other organ mesially situated, and at a distance from the axis, is a double organ and may be the representative of any other organ which is actually double, in which, that is, the two lateral constituents have not coalesced[1].

[1] A similar view was, I find, entertained by that eminently philosophical anatomist, John Goodsir. After giving his reasons for considering that the interspinous bones and mesial fin-rays are elements of the neuro- or endoskeleton, he goes on to say: "With reference to the mesial position and characters of these bones, I would remark, that it appears to me quite permissible, on morphological grounds, to look upon each interspinous bone, with the corresponding fin-ray, as consisting of a right and left actinapophysis mesially united—that is, to consider the right and left halves of which they consist in the young

I think also the view is somewhat too readily accepted, that these fins which I have named are to be regarded in the light of dermal appendages, and that the interspinous bones by which they are continued into the interior are either prolongations from the surface which have insinuated themselves between the deeper structures or are mere membrane-bones, whereby a cardinal distinction is implied between them and the neural and hæmal arches and other parts of the vertebral skeleton.

The ray-bones, it is true, are closely invested by the derma, though they are, at their roots at any rate, covered by the whole thickness of the derma and by areolar tissue as well as by muscular expansions. The superficial muscles of these ray-bones are expanded beneath the skin, and are closely connected with it; and the interspinous bones and their covering muscles are to a great extent separate from the lateral muscles of the trunk, and present the appearance of having been insinuated between them; and in raising the skin from the lateral muscles, the fin and its muscles are readily detached with the skin. Still, the tendinous terminations of the superficial fin-muscles are in some instances[1] inserted into the inter-muscular septa of the lateral trunk muscle, as well as into the skin; and the skin, as is well known, is closely connected with these septa, indeed they are continued into it. And, which is of more importance, the mesial membranous septa, above and below, in the plane of the neural and hæmal spines, are extended to the interspinous bones and establish a direct continuity between the spinous and the interspinous elements (fig. 1).

These membranous septa are of course in each case double, consisting of two planes which have met and coalesced in the middle line. The two planes, passing superiorly from the dorso-lateral and inferiorly from the ventro-lateral parts of

fish, as fundamental elements of opposite sides of the body." He draws an argument in favour of this view from the occurrence of double anal and caudal fins in monstrous fishes. *Anatomical Memoirs*, by Turner, II. 106.

Gegenbaur, *Grundzüge der Vergl. Anat.*, 675, remarks that the ossifications in the rays of unpaired fins commence in pairs and unite into single portions at a little distance from the base. This may be perceived in most fishes; and the two portions usually admit of being easily separated from one another in the whole length of the ray.

[1] *e.g.* the Sole. In the Perch a series of muscular bundles pass, on either side, from the edge of the lateral muscle, being extensions of the fibres of that muscle, to the joints between the fin-rays and the interspinous bones of the great dorsal fin.

the vertebral centres, and enclosing the neural and hæmal canals, extend to the upper and lower margins of the animal, and constitute the boundary lines between the parts formed in the ascending neural and the descending hæmal layers of the two sides. With lateral perforations for nerves and vessels and with lateral intermuscular processes they form, together with the notochord, the primitive skeleton, as instanced in the Lancelet. They give off laterally processes or sheets which form the intermuscular septa of the great lateral muscle; and at the parts where there are no fins (fig. 2) they give off, near the upper and lower edges of the body, oblique processes which enclose the supra- and infra-carinales muscles, while the septa themselves are continued, in the middle line, between the carinales of the two sides to the skin. The dorsal and anal fins occupy the place of the carinales. Accordingly a membranous process from each septum passes on either side of the fin and its muscles, separating it from the edges of the lateral muscle, while the septum itself is continued to, and blends with, the interspinales bones. This may be seen in the transverse sections, and is better seen by stripping off the muscles on the sides from the interspinous bones and the spines of the vertebræ, when both sets of bones will be found to be enclosed in, and connected with one another and with those in front and behind by, one continuous membranous sheet. The blood-vessels and nerves may at the same time be displayed passing on either side of this sheet, in each interval between the spinous processes, to the fin.

In a Pike about a foot long, from which the accompanying drawings were made, I found the large or distal ends of the interspinous bones, both neural and hæmal, composed of cartilage.

In the deeper parts of the mesial septum, above and below, at the lines of junction with, or giving off of, the transverse intermuscular septa, the neural and hæmal spines are formed; and deeper still, where the coalescence of the two sides of the septa is prevented by the presence of the neural and hæmal tubes, the neural arches are formed. In the abdominal part of the animal (fig. 3) the presence of the viscera causes the visceral or descending embryonic laminæ to take a wider sweep,

and prevents the juxtaposition of the laminæ of the two sides and, consequently, of their lining membranous plates which would have united to form the hæmal septum. The abdominal cavity may therefore be said to lie in this septum, and is commonly confluent with the hæmal canal, though not always or at all parts; that is to say, it separates or keeps asunder the two walls of the septum, not merely where they are membranous, but where the hæmal spines should be formed; and the ossifying bars, instead of coalescing as mesial hæmal spines, are lateralized as ribs. The separation generally extends through the hæmal arches as well as the spines, and lays the hæmal and abdominal cavities into one. It affects also the lowest part of the abdominal septum; for the lower margins of the lateral muscles are pressed asunder, and the interval between them is occupied, and the abdominal cavity is closed beneath by the infra-carinales muscles. The foremost interspinous bone, a little behind the anus, with its muscles, commonly closes the abdominal cavity in this direction, and in front of the anus the mesially arranged interspinous series is wanting.

At a variable point however, in front of the anus, usually near to it, are the ventral fins (fig. 3), two in number, one immediately on either side of the middle line, and they are often connected with each other in the middle line. They are quite symmetrical, forming a pair. Each is composed, like the constituents of the anal fin, of an internal portion, that is, a portion projecting inwards between the ventral margins of the lateral muscles of opposite sides, and which is often simple, dagger-shaped and strikingly reminds us of the interspinous bones of the anal fin, and of an external portion projecting beyond the level of the body and consisting of ray-bones with an envelope of skin. These ray-bones are jointed with the internal bones and moved upon them by simple muscles, much as are the rays upon the interspinous bones of the anal fin. Indeed, so close is the similarity, that if a single ventral fin (fig. 5) is held in front of the anal fin, we can scarcely avoid the conviction that it is a continuation of the same series; and, I think, there can be little doubt of this being the case: the chief difference being, that the ventral fin is double, that is, is composed of two symmetrical slightly lateralized portions, instead

of the two portions having coalesced and occupying a strictly median position. It is also more obliquely placed than the anal fin; though the obliquity is in the same direction, viz. slanting from before backwards and downwards. Both these points of difference are attributable to the presence of the abdominal viscera and cavity, which keep asunder the laminæ of the hæmal septum in the manner already mentioned, and cause the interspinous and muscular and ray elements of the fin to be developed on the sides of, instead of in, the mesial line. So that just as the fibrous septa and the ribs lining the abdominal cavity, serially considered, are the ununited lateral elements which in the caudal region combine to form the hæmal spines and the hæmal septum, so the ventral fins, serially considered, are the ununited elements which, behind the abdomen, combine to form the anal fin.

It may be observed that the ventral fins, whether they are placed just in front of the anus or more forwards, derive their nerves, just as do the components of the anal fin, from the ventral branches of the spinal nerves near which they lie.

The pelvic bones of the ventral-fin I regard, therefore, as the serial homologues of the interspinous bones of the anal-fin; each pelvic bone answering to one or more halves of interspinous bones which have been kept separate from those of the opposite side, while their deeper ends have been directed forwards, and their anterior edges inclined inwards towards the middle line. The effect of this has been to throw the hinder margin of the ray backward, and the surface, which in the primitive condition was internal, or opposed to that of the opposite side, backward and upward. Hence, to bring the ventral fins into a position corresponding with the elements of the anal fin, it is only necessary to turn the pelvic bones and the ray from their horizontal to a vertical position, bringing the anterior extremities of the pelvic bones upwards, and their inner edges forwards. The upper surfaces of the ray will now be turned towards each other, or thrown into contact, their inner edges will be directed forwards, and their hinder extremities will point downwards.

The pelvic bones covered by the fin muscles perforate the abdominal wall, their hinder parts, where articulated with the

fin-rays, being external and connected with the skin and the edge of the lateral muscle as well as with the carinales near the middle. In the greater part of their extent they are internal, forming part of the abdominal wall, and separated from the peritoneum only by the fibrous lining of the abdomen. This membrane invests the pelvic bones and muscles and contributes to hold them *in situ.* At the outer edge of the pelvis it divides into three strata, of which one passes externally between the pelvic muscles and the edge of the lateral muscle, and is connected with the skin: a second stratum passes upon the upper or deep surface of the fin, meeting its fellow of the opposite side, and so forming a continuous sheet which strengthens the part and prevents the protrusion of the viscera; while a third or middle stratum passes between the pelvic muscles to the outer edge of the pelvic bone and is blended with it, so establishing a direct continuity between this internal membranous sheet and the pelvic bone, like that which exists between the corresponding sheet in the caudal region and the interspinous bones.

It not unfrequently occurs that the pelvic bone of each side is not so simple as in the Pike, but, as in Clarias anquillaris (fig. 6), presents divisions, a broad portion passing inwards to meet its fellow of the opposite side, while a second portion passes forwards and inwards, and a third passes more directly forwards. These reminders of the pelvic bones of higher animals not improbably represent the incompletely united and diverging interspinous components of the pelvic bone.

The pectoral fins (fig. 4) are situated more laterally than the ventral, being placed at the retiring angle between the ventral and ventro-lateral portions of the great lateral muscle in front, whereas the ventral fins are placed on the mesial side of the ventral portion of the lateral muscle, with the exception of a bundle of the muscle, which, separating itself from the remainder, passes backwards, near the middle line, to the pelvic bone of each side, and lies internal to the projecting part of the fin. Still there seems no good reason to doubt the generally accepted view that the pectoral and ventral fins are serially homologous. If this is so, and the view I have taken of the relations of the ventral fin is correct, it follows that the pectoral

fins are also serially homologous with the anal fin, and the coracoids, like the pelvic bones, answer to the interspinous bones, or portions of them. I say 'or portions of them,' for a comparison of the parts in different fishes (fig. 7) suggests that the series of ossicles intervening between the coracoids and the rays—the brachials of Parker and the carpals of some anatomists—are segmented from the distal part of the interspinous element of which the coracoids form the chief part, and correspond therefore with the distal portions of the pelvic bones.

The collar-like series of bones (fig. 4), which extend from the upper and back of the cranium to the middle line beneath, enclosing the coracoids, lie immediately beneath the skin, yet are connected with the lateral muscle above and beneath, indeed beneath are to some extent imbedded in the ventral portion of that muscle. It is pretty certain that they do not correspond with the coracoids of higher animals, as supposed by Owen. Whether they really deserve the name clavicle which has, with more probability, been assigned to them by Gegenbaur and Parker, further observation must decide.

If the view which I have here given is correct, if the pectoral and ventral fins correspond with the limbs, including their girdles, in higher animals on the one hand, and with the components of the anal fin on the other, and if the osseous elements of that fin and of the pectoral and ventral fins are developed in extensions of the membranous lining of the visceral cavity, in which membrane the hæmal arches and the ribs are developed, it follows that the limb-girdles are in the same internal plane of the visceral wall with the ribs and hæmal arches. They are not hæmal arches or ribs, but formed on a level inferior to, or more marginal than, both. Hence they do not originate in the neighbourhood of the vertebral centres, or have, like the ribs, any direct relation to them, but originate in a more distant part of the visceral layer, and may or may not spread into contact with the vertebræ. The region in which they spring corresponds more nearly with that of the sternum and sternal ribs. Like the several fins of fishes, each is connected with the general muscular system of the animal and has, besides, its own independent muscles passing from segment to segment of the limb.

It follows, further, that each limb of the higher animals

corresponds with a lateral factor or factors of the mesial fin of the fish, and would, if development had proceeded in a similar manner, have united with its fellow into a mesial organ; but the limbs being produced only in the region of the trunk, the (theoretically) apposed surfaces—the palmar and plantar—have been kept apart, and have, as in the case of the lateral fins of the fish, been directed backwards. The direction however of the surfaces, in this instance, is dependent not so much on the inclination of the parts (the girdles) lying in the walls of the trunk, as on the rotation of the outlying parts upon the girdles (see p. 69).

It may be observed also that, according to this view, the median fins—dorsal and caudal, as well as anal—are a series of coalesced lateral limbs, and that on this ground the fish presents some claim to the title of 'neuropod' as well as of 'hæmapod,' which may, when our knowledge of the morphology of the two is more complete, prove to be an additional connecting link between the vertebrate and the invertebrate skeletons.

DESCRIPTION OF PLATE II.

Fig. 1. Transverse section through the tail of a Pike including the dorsal and anal fins: *a.* interspinous bones of the dorsal fin; *b.* interspinous bones of the anal fin. The muscles of the fins are seen on the sides of the interspinous bones. The four divisions of the lateral muscle with its intermuscular septa are seen.

Fig. 2. Transverse section through the hinder part of the abdomen of a Pike; *s. c.* supra-carinalis muscle; *i. c.* infra-carinalis muscle; *a.c.* abdominal cavity.

Fig. 3. Section through abdomen and pelvic bones (*p.*) of a Pike; these are surrounded by the muscles of the fin. The lateral muscles abut upon the fin muscles, but do not at this part extend to the pelvic bones.

Fig. 4. Section just behind the head of a Pike: *s.c.* supra-carinales muscles; *l.*, *l.*, *l.*, lateral muscle; *p. t.* post-temporal bone; *s. cl.* supra-clavicle; *cl.* clavicle; *c.* coracoid with muscles of pectoral fin above and below. The fibres of the lateral muscle are seen to be inserted into the post-temporal and the clavicle.

Fig. 5. The skeleton of the ventral fin of a Pike.

Fig. 6. The skeleton of the ventral fins of Clarias anquillaris.

Fig. 7. The skeleton of the pectoral fin of a Pike.

A COMPARISON OF THE SHOULDER BONES AND MUSCLES WITH THE PELVIC BONES AND MUSCLES. By Professor Humphry, (Pl. III.)

Professor Flower's paper "On the Correspondence between the parts composing the Shoulder and the Pelvic Girdles of the Mammalia," contained in the last Number of this Journal, has induced me to reconsider the subject; and the conclusions at which I arrive do not entirely accord with those of Professor Flower, or with those to which, as he intimates, I myself in some degree formerly inclined. The fact of its being a trite subject, which has been handled by many and able anatomists, proves that it is a difficult one, and therefore interesting; and it is moreover interesting because several of the great principles of animal construction, that is, of morphology and homology, are involved in it. It is quite clear that the fore and the hind limbs are in the main alike; yet a certain amount of dissimilarity pervades every part of them in every animal, adapts each to its special purposes, and is therefore least at the middle, and most marked in the proximal and distal parts of the limbs. The same remarks apply to the limbs in the several members of the vertebrate division. Throughout, variety is grafted upon uniformity. The object of the variety is conformity to the special purposes of the limb, and adaptability to the varieties—that is, the peculiarities—of the several animals. Unfailing in result and regulated by some utterly unknown force, it offers innumerable problems to the homologist, many of which are difficult and some perhaps impossible of solution. When we remember how hard it is correctly to compare the several processes in two distant vertebræ of the same spinal column, although we have the aid afforded by all the gradations presented in the intervening vertebræ, we cannot be surprised that a successful comparison of two limbs placed at opposite ends of the trunk, with different movements and functions, and unconnected by intermediate gradations, has not yet been made. The difficulty of instituting a correct homological comparison is augmented by the fact that the deviations from one another, or of both from a simple and common original, are

very early elements in development, are owing indeed to inherent qualities in the two limbs which are operating from the very beginning of each, and which are working on—and that is a very important point to bear in mind in all developmental and homological investigations—are working on with reference to adaptation to future conditions, and so are prophetic, as it were, of them. For instance, if an alteration in the direction of the circulating current is to take place at a late period of development, the preparation for it is going on throughout the earlier periods of embryonic life; the several parts are forming to effect, and to adapt themselves to, the coming event; and, in the case of the limbs, if rotation or other changes in the hind limb are to occur, differing from those in the fore limb, the developmental processes will, from the earliest period, be directed with reference to them, and will prepare the several parts for them—will prepare, that is, the individual parts for harmonious corporation with the general modification. It may be that they will do so in such a manner as to destroy exact correspondence between the limbs, not only apparently but really, and so render futile the attempt to make an exact homological comparison. If a given muscle passes in one limb on one side, and in another limb on the other side, of a certain muscle or bone, it cannot be said to be strictly homologous in the two limbs, however close its correspondence in other respects may be. Such displacements or variations in the position of corresponding muscles are by no means uncommon. We must be content, therefore, in some cases with establishing a general correspondence, and avoid the error of endeavouring to work out a closer homological relationship than actually exists. These remarks will find their application in the following comparison.

It may be regarded as now sufficiently established, that the extensor and flexor surfaces of the two limbs, of the parts of them, at least, which project from the trunk, answer respectively to one another, and that the radial edge of the fore limb, including the pollex, the outer condyle, and the outer tubercle of the humerus, corresponds with the tibial edge of the hind limb, including the hallux, the inner condyle, and the inner or lesser trochanter of the femur; and, accordingly, that the ulnar

edge of the fore limb, including the inner condyle and inner tubercle of the humerus, corresponds with the fibular edge of the hind limb, including the outer condyle and the outer or greater trochanter of the femur. We may consider that the extensor surfaces of the two limbs form essentially and originally parts of the dorsal plane of the animal, or are an extension of it, and that the flexor surfaces are parts of the ventral plane; and most anatomists accept the view that the two limbs may be considered to have undergone a partial rotation in opposite directions, which has had the effect of throwing the extensor surface of the hind limb forwards, and the extensor surface of the elbow and a considerable part of the fore limb backwards. Many of the apparent discrepancies between the two limbs are thus explained[1].

It is obvious that this rotation does not affect the limbs of all animals alike. In many reptiles it affects them but little; and these animals furnish therefore much aid in the comparison of the limbs. Moreover, it does not affect all parts of each limb to an equal extent. The distal part of the fore limb in some mammals and in birds retains the primitive direction of its surfaces; and in most mammals it undergoes a rotation in a direction opposite to that of the rest of the limb, and like that of the hind limb; for its dorsal surface is, like that of the hind foot, directed forwards, and its ventral surface backwards. The Bat offers an interesting example of the rotation of an entire limb in a contrary direction to that which is usual, the whole flexor or ventral aspect of its hind limb being turned forwards.

The question arises, is this rotation of the limbs participated in by their respective trunk-segments or girdles, or by any parts of them? With regard to the lower portions of these girdles—the coracoid elements in the shoulder, and the ischiatic and pubic elements in the pelvis—I am not aware that such rotation has ever been supposed, and the evidence is greatly against it. It is pretty clear that the respective external and internal surfaces of these elements in the two girdles answer to one another, and to the external and internal planes of the visceral laminæ in which they are formed. This, it must be admitted, is strong presumptive evidence against any rotation

[1] See my *Observations on the Limbs of Vertebrate Animals*, p. 16.

of the superior elements of the girdles (the scapular and iliac parts). The several parts of each girdle are, in their early and their cartilaginous conditions—and indeed to a great extent throughout life—in one piece, unsegmented, the distinction being caused by the formation of independent centres of ossifications, which after a time become blended, rather than upon any real separation or segmentation. This at least is the case except where the coracoid becomes separate. It is therefore difficult to suppose that the scapula and ilium can undergo a rotation which is not participated in by the coracoid and ischium. Moreover, it is pretty certain that the external surfaces of the lower parts of these two—the articular surfaces and the spaces just above the articular surfaces upon which the prolongations of the triceps brachii and of the rectus femoris extend—answer to one another.

The only question, then, which remains—and this I believe, though it has not been quite clearly stated, is the only one which has really been raised—is whether a rotation in opposite directions takes place in the upper parts of the scapula and ilium, turning that which was the dorsal surface of the blade of the scapula backwards, or backwards and inwards, and that which was the dorsal surface of the blade of the ilium forwards; so that the dorsal aspect of the scapular blade with its muscles answers homologically to the surface of the ilium which is covered by the iliac muscle and that muscle, while the subscapular aspect of the scapular and the subscapularis muscle are homologous with the gluteal aspect of the ilium and certain of the glutei muscles.

Now I am not aware that there is any example of a corresponding twist in the length—that is, between the two ends—of any of the bones of the limbs, though something of the kind has been assumed with regard to the humerus by M. Martins of Montpellier. The rotation appears in each instance to affect one or more entire bones, and not to be caused by a twist in any one. Thus the entire radius is rotated upon the humerus and the ulna, and the entire humerus is rotated upon the scapula. It seems less probable that a bone, or part of a bone, formed in the wall of the trunk and connected on all sides with surrounding adherent tissues, should undergo a rotation similar to that

which has been supposed. I think, moreover, the peculiarities in the two limbs may be explained and harmonized without resorting to any such explanation.

In the paper already referred to, Professor Flower truly observes that "in every mammal both scapula and ilium may be resolved into a bar or rod of three-sided or prismatic form" with the two extremities placed dorsally and ventrally. "The dorsal or upper extremity is capped by the supra-scapular epiphysis in the shoulder-girdle, and by the corresponding supra-iliac epiphysis in the pelvic girdle. The ventral or inferior extremity enters into the formation of the glenoid or the cotyloid articular cavity, as the case may be, and joins with the *coracoid* or the *ischial* element of the girdle." But Professor Flower goes on to make a distinction between the direction of the borders and surfaces of this prism in the ideal and the human scapula and pelvis, a distinction based upon the theory of the rotation of these parts, in which I am unable to concur; for, as just stated, the ideal and the human girdles, and consequently the ideal and the human scapula and pelvis, appear to me to correspond very closely.

Take the ilium of an Echidna (fig. 1) or a Kangaroo, in both of which—and indeed in many other instances—the prismatic form is well presented. Of the three margins or borders one is external and descends to, or nearly to, the upper edge of the cotyloid cavity. It separates the antero-external surface which is covered by the *iliacus internus* muscle from the postero-external surface, which is covered by the *glutei*. It represents therefore the blade and fore part of the crest of the ilium. Its position and appearance and relation to the *iliacus internus*, which Mr Mivart and others on good grounds regard as the homologue of the *supra-spinatus*, are strongly suggestive of its answering to the spine of the scapula, and we shall find other reasons confirmatory of that view. The anterior margin or border is continuous with the linea ilio-pectinea of the os pubis, separates the antero-external or iliacus surface from the internal or ventral surface and is the anterior, or inner, or ilio-pectineal edge of the ilium. The hinder margin or border separates the postero-external or gluteal surface from the internal or ventral, and represents the hinder or sciatic edge of the ilium. If I am

right in believing, as I think I can show that I am, that the external ridge is the representative of the iliac blade and fore part of the crest, and that it answers to the spine of the scapula, then it follows that the anterior or ilio-pectineal ridge answers to the supra- or rather pre-spinal portion and margin of the scapula, and that the posterior or sacro-sciatic ridge answers to the back part and the posterior border of the scapula. It follows also that the ventral surface of the ilium, that is the surface behind the ilio-pectineal line, including the space for articulation with the sacrum, corresponds with the ventral or subscapular surface of the scapula; and the dorsum or gluteal region of the ilium corresponds with the infra- or rather post-spinal region of the dorsum of the scapula, *i.e.*, the part behind the spine and extending over the hinder aspect of the spine.

In the Kangaroo and Echidna the prismatic form of the ilium is well preserved; the three ridges are of nearly equal prominence, and the three surfaces are of nearly equal size. This is also the case in some rodents, as the Beaver (fig. 2). In other instances the ridges are unequally developed, and the surfaces consequently are of unequal size. In the Hare (fig. 3) and Rabbit the ilium is comparatively flat and broad in consequence of the outgrowth of the anterior and posterior ridges, while the external ridge is scarcely perceptible. In this instance the iliacus internus lies upon the outer surface of the anterior or ilio-pectineal border of the ilium, rather than upon the inner surface of the outer border; and the ilium unmistakably resembles the scapula in those animals in which the spine is suppressed, as the Horse (fig. 5). This is however rather exceptional. In most mammals the anterior ridge is but little developed, and is recognized only as the 'ilio-pectineal line' or 'brim' upon the inner surface of the ilium, separating the true from the false pelvis. The external ridge becomes developed in very varying degrees. Still suppressed (the ilium preserving much of its primitive form) in carnivora, its upper margin runs out into an overhanging crest in the Wombat and in ruminants; whereas in pachyderms, monkeys and Man it grows out in its whole length forming the part of the blade and crest of the ilium which lies above and anterior to the ilio-pectineal line and the articulating surface for the sacrum. The iliacus

internus now rests upon the inner aspect of this broad ridge, and the conformation of the ilium corresponds closely with that of the scapula of the Echidna.

The posterior or sacro-sciatic ridge—which bears relations to the ischium and to the large nerves and blood-vessels of the hind limb corresponding to the relations which the hinder border of the scapula bears to the coracoid in ovipara, and to the large nerves and blood-vessels of the fore limb—grows out in varying degrees; and its upper part is often produced backwards into a 'posterior spine' overhanging the sacro-sciatic notch and resembling the 'posterior angle' of the scapula.

Whatever value may be attached to the disposition of the nutritious foramina in the two bones—and it cannot be considered to be unimportant—it certainly favours, in rather a remarkable manner, the view I have taken. These foramina will be found pretty regularly, in man at any rate, in five sets in each bone. *First*, in the ilium on the anterior aspect of the iliac blade near the linea-ilio-pectinea, and in the scapula on the anterior aspect of the spine near the anterior or pre-spinal ridge: *secondly*, in the ilium on the posterior or gluteal aspect of the blade nearly opposite the preceding, and in the scapula on the posterior aspect of the spine in a corresponding situation. *Thirdly*, in the ventral surface of the ilium behind the ilio-pectineal line, and in the ventral surface of the scapula behind the supra- or pre-spinal ridge: *fourthly*, in the inferior edge of the iliac blade just above the acetabular surface and in the inferior edge of the scapular spine just above the glenoid surface; *fifthly*, in the posterior or sciatic edge of the ilium near the acetabulum, and in the posterior edge of the scapula near the glenoid cavity. In the lower animals the nutritious foramina are less constant than in man; but when they appear they are in or near one or other of the situations indicated.

The origin of the *rectus femoris*, from the anterior border of the ilium, and of its homologue the long portion of the *triceps* from the hinder border of the scapula, may be esteemed an argument in favour of the correspondence of these two borders, and so of the rotation of the scapula and ilium in opposite directions. But it must be remembered that the triceps arises from the outer surface of the scapula as well as from the

hinder border, and that the rectus extends from the anterior border of the ilium upon its outer surface above the acetabulum. The origin therefore of the two may be traced to corresponding points upon the external or dorsal aspects of their respective bones; and the one has been turned backwards upon the hinder border of the scapula, while the other has been turned forwards upon the anterior border of the ilium, in consequence of the rotation of the extensor surfaces of the limbs in those directions[1]. This difficulty becomes therefore an argument in favour of the view that the rotation of the parts of the limbs that are free from the body is not accompanied by a corresponding rotation of the parts lying in the visceral wall.

An important difference between the two limbs in mammals, which has a material effect upon the inferior components of the girdles, is to be found in the position of the limbs, or rather of the proximal components of the limbs, with regard to the respective girdles. The hind limb impinges upon the pelvis *laterally*. The direction of forces from it to the pelvis is consequently *in*wards as well as upwards; and this is associated, as a general rule, with the completion of the bony arch beneath which is effected by means of the great development of the ischium and os pubis, and the meeting of the pubic and perhaps the ischiatic bones of the two sides at the symphysis. The fore-limb including the humerus in mammals, excepting monotremes, is placed *beneath*, that is, in a line with the scapula, and the forces are consequently directed from it to the scapula more vertically than in the case of the hind-limb. The lower elements of the girdle are accordingly comparatively abortive, and do not approach the middle line. In monotremes, however, as well in the inferior classes, the fore-limbs are more sprawling, the humerus runs out almost horizontally from the trunk, the glenoid cavity is more lateralized and the direction of forces from the limb is more *in*wards. Hence the lower elements of the girdle acquire a development more or less corresponding with those of the pelvis, and abut upon one another or upon the sternum.

Another, but less important difference, to some extent

[1] In Manis the *triceps* arises from the whole length of the spine of the scapula, as well as from the hinder border. *Journal of Anat.* IV. 38.

associated with the preceding, is a rotation in opposite directions in the two girdles, which may be connected with the rotation in opposite directions in the two limbs already referred to. It must, however, be clearly understood that this is altogether different from the rotation of the ilium and scapula upon their longitudinal or vertical axes which I have been arguing against. The rotation to which I now refer is one upon a transverse axis drawn through the two acetabula in the case of the pelvis, and through the two glenoid cavities in the case of the shoulder[1].

In what we may suppose to be the primitive condition, as illustrated by the Chameleon and many other reptiles, the pelvic and scapular girdles are almost vertical. The nearly straight and flat ilium and scapula descend to the articular spaces whence the two inferior elements of either girdle pass inwards and downwards enclosing perhaps a space between them. The arm and thigh-bones run out nearly horizontally so that the axes of the two arm-bones, if prolonged inwards, would meet and form one horizontal straight line passing through both glenoid cavities and constituting the axis of the rotation of the scapula to which I refer. In like manner the axis of pelvic rotation is the axes of the two thigh-bones traversing the acetabula and meeting in the middle line. Now, the developmental rotation of the limbs takes place upon these axes, and is accompanied, or is often accompanied, by a corresponding rotation of the girdles upon the same axes. Thus when the hind-limb rotates so that the upper or extensor surface turns forwards, and the under or flexor surface backwards, there is a rotation, not of the ilium upon its vertical axis, but of the whole pelvis, turning the upper edge of the ilium forwards and the hinder edge of the ischium backwards. Also when the fore-limb rotates, so that its upper or extensor surface turns backwards, and its under or flexor surface turns forwards, the rotation affects, not the scapula upon a vertical axis, but the whole girdle upon a horizontal axis, turning the upper edge of the scapula backward and the coracoid forwards.

I should observe, however, that this turning of the girdles

[1] It is a change of like kind with that which brings the pelvic bones of the fish into a horizontal position from the vertical direction of the interspinous bones (p. 63). It is however less in extent; and in the shoulder girdle usually, and in the pelvic girdle sometimes, is in an opposite direction to that in the fish.

upon a transverse axis is very uncertain, and does not always correspond in amount or even in direction with that of the limbs. The pelvis, for instance, in rodents, in birds and the Frog undergoes very nearly a quarter turn; and the ischium is in the Beaver thrown nearly, and in the Great Anteater quite, into contact with the caudal vertebræ, and in Aï it is anchylosed with them; and in this animal and most birds, it sends forwards a process which nearly or quite blends with the hinder margin of the ilium, and converts the sacro-sciatic notch into a hole. In Pteropus the two ischiatic bones are united together in the middle line behind the tail. In the Kangaroo the turn hardly takes place at all; and in most saurians the pelvis is turned in a contrary direction, the ilium being slanted backwards. In the shoulder girdle the rotation is usually slight. In the through-bred Horse it is as marked as in any instance that occurs to me. Some of the elements are however not uncommonly slanted, *i.e.*, grow out from the glenoid area in the direction indicated without the other or others being much altered. Thus in the bird the scapula is slanted backwards, although the coracoid maintains its primitive direction inwards; and in all the mammals in which the humerus is directed downwards from the shoulder, the inferior elements of the girdle, concentrated in the small short coracoid, are directed forwards, although the scapular blade is but little altered from its primitive upward direction. It is here worthy of remark that as the backward projecting ischium of Aï and some birds comes into contact with, and unites with the backward growing hinder or sciatic spine of the ilium, so in Aï does the forward projecting coracoid unite with the forward growing anterior or prespinal edge of the scapula and encloses the supra-scapular hole.

A word or two respecting the clavicle. Imbedded in the superficial stratum of muscles and stretching across from the acromion or overhanging projection at the inferior extremity of the spine, *i.e.*, the external ridge, of the scapula towards the median line, where it unites with its fellow by anchylosis or by ligament, and perhaps impinges upon the sternum, it seems to answer to the tendinous fibres called Poupart's ligament which, lying in or forming part of the superficial muscular stratum, stretch across from the anterior iliac spinė, or over-

hanging projection, of the external ridge of the ilium, towards the median line. There they not only impinge upon and are implanted into the pubic bone, but some of the fibres are commonly united in the median line with those of the opposite side. May not the interclavicle or episternum, which in monotremes and saurians subtends the clavicles, and, extending beneath the sternum, gives origin on its sides to the *pectorals*, be regarded as an ossification of tissue homologous to that which subtends Poupart's ligaments and, extending beneath the symphysis pubis, gives origin on either side to the fibres of the *gracilis*. Further, are not both clavicle and Poupart's ligament serially homologous with the intermuscular bones in the blended obliqui externi and recti of saurians where they occupy a plane underlying, that is, superficial to, the costal cartilages.

I conclude therefore that the following parts are respectively homologous. The *pre-spinal ridge* of the scapula, which forms the floor of the pre-spinal fossa, and the *linea ileo-pectinea* of the ilium:—the *spine* of the scapula with the *acromion*, and the *fore* part of the *blade* and *crest* of the ilium with its anterior *spine* or *angle*:—the *post-spinal* part of the scapula, which forms the floor of the post-spinal fossa, and the *hinder* part of the *blade* and *crest* of the ilium; the *posterior angle* of the scapula corresponding with the *posterior 'spine'* or *angle* of the ilium:—the *hinder border* of the scapula, and the *hinder* or sciatic *border* of the ilium:—the *inner* or ventral surface of the scapula, and the *inner* or true pelvic surface of the ilium, including the surface for articulation with the sacrum:—the *coracoid*, which in reptiles divides into coracoid and precoracoid with an intervening *fenestra*, and the *ischiatic* and *pubic bones* with the *obturator* hole:—the *clavicle*, and *Poupart's ligament*:—the *interclavicle*, and the *fibrous tissue* beneath the symphysis pubis.

In order to obtain a correct apprehension of the relations of the muscles in the two limbs, it is necessary to bear in mind that the shoulder and pelvic girdles are developed in and form parts of the descending or visceral plates of the embryo, a half of each girdle on each side, that they lie in the innermost stratum of those plates and are connected with the several strata of muscles in them; and that the limbs, as they emerge from the

ventro-lateral regions of those plates, carry out with them prolongations of their muscles which extend to greater or less distance and acquire attachment to the bony framework of the limbs. These muscles may be divided into a dorsal and a ventral series which converge respectively from the dorsal and the ventral aspects of the trunk upon the dorsal and the ventral aspects of the limbs, and form an external or superficial sheet upon them. A deeper sheet is formed by the muscles passing from the half-girdle of each side to its limb. These also constitute a dorsal and a ventral series according to their connection with the part of the girdle above or beneath the point from which the limb springs.

In the primitive state the extensor aspect of each limb is dorsal, and the flexor aspect is ventral; and the dorsal muscles both from the trunk and the girdles pass upon the extensor or dorsal aspect of the limbs, and the ventral muscles both from the trunk and the girdles pass upon the flexor or ventral aspect of the limbs. In short, the several layers from the skin inwards are continuations of the several layers of the embryonic laminæ and they are continued dorsally, ventrally, anteriorly and posteriorly upon the corresponding aspects of the limbs.

Thus far the matter is simple enough; and it is on the whole easy to refer the several muscles to their respective dorsal and ventral groups both in the trunk and the girdle series. Complications however arise from various causes which create some difficulties, especially in the sub-division of the several groups and in the comparison of these subdivisions in the two limbs.

First, the members of the dorsal and ventral series do not always rigidly adhere to their respective aspects of the limb. The lateral or marginal muscles especially may overlap their appropriate margins and extend from a dorsal to a ventral aspect. Thus the *latissimus dorsi*, which obviously belongs to the dorsal series and often blends with the triceps extensor cubiti, is not unfrequently continued on to the flexor aspect of the forearm: in Manis[1] it is continued into the *flexor digitorum;* and in Unau[2] the *tibialis anticus* is continued into the *flexor digitorum pedis.*

[1] *Journal of Anat.* IV. 35. [2] *Journal of Anat.* IV. 67.

Secondly, a muscle may be attached higher or lower, or near to one or other margin of the dorsal or ventral aspect of its bone. The *latissimus dorsi*, for instance, in the bird passes, on the radial side of the humeral part of the triceps, to the radial edge of the humerus; in saurians it passes between the divisions of the triceps to near the middle of the dorsal aspect of the bone: whereas in mammals it passes on the ulnar side of the triceps to the ulnar margin of the bone. True, this kind of variation does not often occur to any great extent; but it is not always easy to make sure respecting it; and where it does occur it throws much doubt upon homological arrangement.

Thirdly, although in the distal segments of the limbs the dorsal and ventral surfaces, as well as the radial or tibial and the ulnar or fibular margins, are pretty clearly defined, and the distinction between the dorsal and the ventral groups of muscles is comparatively easy, yet in the proximal segments, where there is only one bone in each limb, this is not the case, especially in the middle and towards the upper ends of the segments. In many reptiles and birds, and in monotremes, it is true, the upper end of the humerus is flattened, and the dorsal and ventral surfaces are separated by anterior and posterior ridges which terminate in the anterior or radial and the posterior or ulnar projections or tubercles; but in other animals the dorsal or extensor surface is increased at the expense of the ventral or flexor surface. The marginal lines and tubercles are pressed or folded to the flexor side and come almost into apposition, leaving between them, it may be, little more than the interspace for the biceps tendon. It is difficult in some such cases to define how much appertains to the dorsal and how much to the ventral surface; and the difficulty is increased by the fact of the muscular attachments overstepping what may morphologically be called their proper limits. In the case of the femur we rarely meet with so equal a division into dorsal and ventral aspects as is seen in the humerus of the animals just mentioned. Here too the extensor is increased at the expense of the flexor surface, which is partly in consequence of the flexor muscles of the leg having little or no connection with the femur: the lateral margins of the shaft are folded almost into contact in the linea aspera, and sometimes, as in saurians,

the lateral tubercles are blended into one ventrally-placed spine.

Fourthly, the rotation of the fore and hind limbs in opposite directions necessarily tends to increase the confusion. It does so, not simply by reversing the relative position of the muscles, but, I think, also by impressing in some instances an opposite developmental tendency upon the muscles of the two limbs and causing them to be directed in each towards that margin or that tubercle which will be most appropriate to their action in the final position of the limb. This seems to me to be an explanation of the fact which has been the source of so much embarrassment to homologists, and which indeed precludes an exact serially homological comparison of the fore and hind limbs, viz., that the muscles from the dorsal aspect of the scapula pass to the radial tubercle of the humerus, while those from the dorsal aspect of the ilium pass to the fibular tubercle of the femur. Those tubercles in the two limbs, though homologically different, yet in size, in position, in function, and to some extent also in muscular relations, are made to answer to one another. That the course of muscular transformation in the embryonic plasma should be thus conformed to the future requirements of the two limbs is no more than other modifications would lead us to expect; especially when we find the dispositions just referred to, of the latissimus dorsi in birds and saurians, and of the tibialis anticus in Manis, presenting instances of a like kind in corresponding limbs.

Fifthly, more numerous are the infringements of serial homological order presented by the blood-vessels and nerves passing to the several muscles in the two limbs. The blood-vessels are perhaps the most conformable of animal structures, which is no more than would be expected from their mode of formation and their disposition, and they give us very little help in our present task. From the nerves, more of definiteness in arrangement and tenacity of serial homology is to be expected, than from the blood-vessels. Still, when we mark the differences in the disposition of the nerves in the two limbs, and the intricate manner in which the nerves passing to the limbs are respectively blended in the cervical and the lumbo-sciatic plexuses, our confidence in any readily available assistance from

this quarter in cases of difficulty, in the present state of our knowledge, is very much shaken. Further investigation and more accurate dissection may exhibit more harmony in the disposition of the nerves in the two limbs than we can at present trace, and may not improbably lead to a modification of some of the views to which we are now led.

I will now endeavour to make a comparison of the muscles about the shoulder with those about the hip, in accordance with the principles I have laid down; and first, of the muscles passing from the trunk to the limbs. These may be arranged into dorsal, ventral, anterior and posterior. The last two might be described as lateral muscles, being derivatives from the great lateral system of muscles which is so fully developed in the fish.

The DORSAL trunk-muscles pass from the vertebral spines to the limbs, and consist of a superficial and a deep layer.

The superficial layer is composed of the *trapezius*, the *latissimus dorsi* and the *gluteus maximus*. These form a nearly continuous series; and their several parts, that is to say, their several component portions passing from the several vertebral spines, may be regarded, generally speaking, as serially homologous[1]. Each of them is therefore, in this sense, serially homologous with the others; and the attachment of the *latissimus dorsi* to the crest of the ilium is a repetition of the attachment of the *trapezius* to the scapula. The similarity, however, between the *latissimus dorsi* and the *gluteus maximus* is very evident. The former passes over the hinder angle of the scapula, deriving fibres from it, lies on the ulnar side of triceps, and is implanted into the dorsal aspect of the humerus usually near the ulnar ridge. It is often blended, to some extent, by means of connecting slips with the triceps; and prolongations

[1] I use the phrase 'serially homologous' in this and some other places to indicate not merely the corresponding muscles in the upper and lower limbs, but also the muscular bundles arising from corresponding parts of the vertebræ in different regions of the column. In this sense the several bundles of the trapezius are serially homologous with one another and with the several bundles of the latissimus dorsi and the gluteus. The destinations of the collective bundles of the latissimus dorsi and of the gluteus indicate a more special serial homology between these two muscles than between either of them and the trapezius. The whole are in a general way serially homologous, whereas some of them only are specially so.

of it occasionally pass to the ulnar condyle, or on to the flexor aspect of the forearm, being traceable as far as the wrist[1]. The *gluteus* in like manner frequently derives origin from the posterior angle of the ilium, lies on the fibular side of the quadriceps, and is implanted into the dorsal aspect of the femur usually, near to the fibular line. It sends a fascial expansion over the quadriceps extensor, and is frequently continued on to the flexor aspect of the leg chiefly on the fibular side as far as the ankle.

The deeper layer of the dorsal series, which is probably a segmentation from the trapezius, consists of the *rhomboids*, to which are sometimes added the *masto-scapular* and the *occipito scapular*[2]. These have no distinct representatives in the hind-limb.

The VENTRAL trunk-muscles to the fore-limb are the *Pectorals*. They pass on the radial side of the flexors of the forearm, and are attached upon the ventral aspect of the humerus near the radial line, which is here projected into what is called the pectoral crest. The superficial stratum, arising along the ventral mesial line from the sternum and the tissue superficial to it, from the episternum and clavicle, when they are present, descends lower than the rest of the muscle, and is not unfrequently continued upon the radial side of the flexor surface of the forearm. It is represented in the hind-limb by the *gracilis* which, arising from the symphysis pubis and the tissue superficial to it, extends, upon the flexor surface of the leg, often to the ankle and to the

[1] In the Frog the *latissimus dorsi* joins the hinder edge of the *dorsalis scapulæ*, in which are blended the *infra-spinatus* and *teres minor* and *major*, and its tendon passes with it on the radial side of the *triceps* to near the radial edge of the humerus just beneath the deltoid. In the bird (Owl) it arises in two separate small slips, one above the other, which pass together, between the scapular and humeral portions of the *triceps*, to the dorsal aspect of the pectoral crest just beneath the *deltoid;* it is separate from the *teres major*, which is large and runs to the dorsal aspect of the ulnar tubercle. In the saurian (Scinc) it runs between the two scapular origins of the *triceps*, then between the two humeral origins, to near the middle of the dorsal aspect of the humerus: the teres major is separate from it and disposed as in the bird. In the two-toed Ant-eater it crosses beneath the flexor aspect of the humerus to the radial edge beneath the pectoralis major (*Journal of Anat.* IV. 34, see there remarks on the disposition of this muscle).

Indeed, both this muscle and the *gluteus maximus* are exceedingly variable in size, in the range of origin and insertion, and in the extent of limb covered by them. See disposition of Gluteus in Manis, Vol. IV. 52.

[2] The *cleido-occipital* and *cervico-humeral* are also occasional segmentations from the trapezius, but do not belong to the deeper layer.

inner edge of the tibia. Deeper strata of the pectoral arising from the ribs cross the superficial fibres and take a more upward direction towards the upper part of the pectoral crest, and may extend to the coracoid or the clavicle. These, or parts of them, are sometimes segmented as distinct muscles—the *pectoralis minor*, and the *subclavius*—The latter, as shewn by Prof. Rolleston, in the bird passes on over the coracoid to the humerus, constituting the *pectoralis secundus*. It still conforms to the disposition of the members of the ventral group in being inserted on the ventral aspect of the radial tubercle. These deeper strata do not appear to have any distinct representative in the hind-limb. It may be that they are there merged in the adductor series.

The POSTERIOR trunk muscles passing to the fore-limb are the *serratus magnus*, arising from the ribs behind and beneath the scapula, and inserted into the hinder or upper border of the bone, and the *sterno-coracoid* which passes from the deeper surface of the sternum to the deeper surface of the coracoid in the animals in which the coracoid and sternum are in contact. The brachial vessels and nerves pass between these two muscles.

The ANTERIOR trunk muscles are the *levator scapulæ*[1] and *omohyoid*, which are inserted into the anterior border of the scapula[2].

The fibres of the *quadratus lumborum* which pass from the transverse lumbar processes to the hinder part of the anterior margin of the ilium are probably the serial homologues of the *levator scapulæ*. The other lateral scapular trunk-muscles do not appear to be represented by any distinct pelvic trunk-muscles; but there are pelvic trunk-muscles—the *sacro-lumbalis*, *ischio-coccygeus*, &c., which have no distinct representatives in connection with the scapula.

There remain the *psoas* muscles, the relations of which to the quadratus lumborum and iliacus internus indicate that their representatives if present would appear anterior and internal to

[1] The *levator scapulæ* and the *serratus* are often continuous, so that the two might be described as one muscle lying before, behind, and beneath the scapula.

[2] It is worthy of remark that in Echidna the omohyoid preserves its usual relations, being inserted beneath the supra-spinatus into a faint ridge which represents the anterior border of the scapula, and resembles the ilio-pectineal line of the human ilium.

the levator scapulæ and the supra-spinatus. The *cervico-humeral* does not usually answer to the conditions; but it is extremely interesting to note that in Phoca (Vol. II. 299) one portion of this muscle extends from the transverse process of the atlas to the anterior angle of the scapula, and overlaps the supra-spinatus, thus presenting very close homological relations to the *psoas parvus;* and in that and some other animals an extension of the *levator scapulæ* into the fascia over the *supra-spinatus* remind us of the relations of the *psoas magnus* to the *iliacus internus.*

We come now to the muscles passing from the shoulder and the pelvic girdles to the humerus and the femur. They may be divided into dorsal and ventral, anterior and posterior, which are disposed accordingly upon the dorsal and ventral, the anterior and posterior aspects of the shoulder-girdle and humerus, and the pelvic girdle and femur.

The DORSAL series of girdle-muscles in the fore-limb consists of the *deltoid, infra-spinatus* and *teres minor.* The *deltoid* passes from the spine of the scapula, the acromion, and the outer part of the clavicle to the extensor or dorsal aspect of the radial line of the humerus, nearly opposite to the attachment of the pectoral. It sometimes (Orycteropus) extends with the flexor of the forearm to the radius, and in Manis it joins the *supinator radii longus*[1]. It seems to be represented by the *sartorius,* which arises from the anterior iliac spine often extending upon Poupart's ligament, and is inserted either into the dorsal aspect of the tibial line of the femur internal to the vastus internus, or passes down to the tibia, meeting the gracilis much as the deltoid meets the pectoral. I may observe that the occasional

[1] *Journal of Anat.* IV. 36 and 40. The *extensor plicæ alaris* of the bird may also be regarded as a derivation from the deltoid to the supinator and the radial edge of the wing. The clavicular and the scapular parts of the deltoid are not unfrequently separate; and the latter sometimes blends with the triceps (see Pteropus, *Journal of Anat.* III. 305). This part may be represented by the *tensor vaginæ femoris,* which is sometimes inserted into the dorsal aspect of the femur.

In Manis, the *sartorius, tensor vaginæ femoris,* and *gluteus maximus* are continuous; and in that animal the *supinator longus* extends up to the spine of the scapula, displacing the fore part of the *deltoid,* reminding us of the connection of the supinator with the deltoid in the bird, and suggesting the serial homological relation of that connecting portion and of the *extensor plicæ alaris* with the sartorius. See Disposition of Sartorius, in Vol. IV. 55.

extension of the *gracilis* upon the inner, and of the *sartorius* upon the outer part of Poupart's ligament, remind us of the extension of their homologues—the *pectoralis major*, and the *deltoid*—upon the inner and outer parts of the clavicle. The relation thus established between the *gracilis* and *sartorius* with the *external oblique*, resembling that of the *pectoral* and *deltoid* with the *cleido-mastoid* and *trapezius*, and the occasional continuity of the *sartorius* and *tensor vaginæ femoris* with the *gluteus magnus*, suggest that all these muscles appertain to, and are segmented from, one continuous muscular stratum passing from the anterior and posterior median lines of the trunk upon the limbs[1].

The *infra-spinatus* and *teres minor* are represented by the *gluteus medius and minimus*. The segmentation in each instance is often incomplete. The two in the fore-limb are sometimes formed into one *dorsalis scapulæ*. The difficulty in the comparison of these muscles caused by those in the fore-limb passing to the radial tubercle, while those in the hind-limb pass to the ulnar trochanter, has already been alluded to, and I hope, removed.

The *teres major* passes from the hinder angle of the scapular on the ulnar side of the triceps, to the ulnar edge of the humerus, and is the POSTERIOR girdle-muscle of the fore-limb. It is commonly placed upon the dorsal aspect of the angle though it sometimes lies upon the ventral aspect[2]; and it sometimes passes on to the dorsal aspect of the humerus. It is often related in its whole length to the latissimus dorsi, yet is sometimes quite separate from it. It is sometimes (Frog) apparently blended with the infra-spinatus and teres minor in the dorsalis scapulæ; though when separate its nervous supply is from a different source (the subscapular). Its general relations therefore give it rather a dorsal character. This makes me hesitate to compare it with the *pyriformis* in the hind-limb, which, arising slightly from the ventral aspect of the posterior spine of the ilium, and largely from the same aspect of the transverse processes of the sacral and caudal vertebræ, and

[1] See remarks in paper on Anatomy of Orycteropus in Vol. II. 298.

[2] In Manis I found it arising from the hinder border and inner surface of the scapula internal to the triceps. *Journal of Anat.* IV. 36; also in Orycteropus, II. 300.

inserted into the ventral aspect of the fibular trochanter, has a more ventral situation. Yet the position of the two muscles on the whole, their relations to the hinder borders of the scapula and the ilium, to the great vessels and nerves of the two limbs (these passing over the inner aspect of the muscle in each instance) and to the circumflex and dorsalis scapulæ vessels in the one case, and the glutæal vessels in the other (these vessels crossing in front of the respective muscles and passing between them and the borders of the scapulæ and ilium)—added to the fact that the pyriformis is often absent, or blended with the gluteus medius, just as the teres major is often blended with the infra-spinatus—indicate a correspondence that cannot be overlooked[1].

The ANTERIOR girdle-muscles are the *supra-* or *pre-spinatus* in the fore-limb and the *iliacus internus* in the hind-limb. The former passes from the pre-spinal fossa of the scapula, over the fore part of the coracoid and shoulder-joint, to the edge of the radial tubercle of the humerus; and the latter passes from the pre-spinal fossa of the ilium, over the precoracoid and fore part of the hip joint, to the edge of the tibial trochanter of the femur[2]. In the fore-limb of the saurian the *prespinatus* extends upon the broad precoracoid, and in the hind of the same animal, the *iliacus internus* extends upon the broad pubic bone, or pre-ischium.

The VENTRAL girdle-muscles pass from the lower elements of the girdles, the coracoid and the ischiatics, to the ventral aspect, and chiefly to the ulnar and fibular sides of the humerus and femur. They are formed chiefly in the fore-limb by the *coraco-brachials* and, in the hind limb, by the *adductors*, including the *pectineus*, and the *external obturator*. All these vary in size and number in accordance with the varying development of the bones from which they spring[3], and

[1] The remarkable muscle in saurians called *Pyriformis*, which arises by a long thick fleshy belly from the under surface of the transverse processes of several caudal vertebræ, and is inserted by a strong tendon on the tibial side of the single ventrally placed trochanter, and which detaches at right angles a tendon to the fibula, presents strong claim to its name, and is probably either the homologue or the serial homologue of the pyriformis of mammals.

[2] The *iliacus* frequently extends lower down on the femur, and the *supraspinatus* occasionally (Phoca) extends lower down on the humerus.

[3] In Cyclothurus and Manis where there is no appreciable coracoid there is no coraco-brachialis.

are accordingly more numerous and larger in the hind-limb than in the fore-limb, and in the fore-limb they are larger in ovipara and monotremes than in ordinary mammals. When the coraco-brachials are extensively disposed along the humerus, they are pierced by the brachial artery, passing from the extensor to the flexor aspect of the limb; just as in the hind-limb the adductors are pierced by the femoral artery passing from the front of the thigh to the ham[1].

These remarks apply to the coraco-brachials and adductors which arise from the *exterior* of the coracoids and pub-ischiatic. But there coraco-brachials which arise from the *internal* surface of the coracoids in the animals where those bones are prolonged to the sternum, and which either join the external coraco-brachials, and pass with them down the humerus; or which ascend to the ulnar tubercle of the humerus[2]. There is often—and that is the case in saurians—one of each, that is to say, there is an *inferior internal coraco-brachialis*, which passes from the internal surface of the coracoid to the shaft of the humerus, and a *superior internal coraco-brachialis* which passes from the inner surface of the coracoid to the ulnar tubercle of the humerus. The latter is the larger, and is closely connected, or continuous, with the *subscapularis*. I apprehend that these internal coraco-brachials (one or both) are represented by the internal ischio-femoral (the *obturator internus* or the lower part of it) which passes from the internal surface of the ischio-pubic bone and the obturator ligament to the ventral aspect of the fibular trochanter. The upper bundles of the *obturator* which are expanded upon the ventral surface of the ilium, beneath the ilio-pectineal line, appear to correspond with the *subscapularis*. The *quadratus femoris* is merely a segment of the *adductor magnus;* and if any special homologues of the *gemelli* are to be sought, it must be in the coraco-brachial groups.

[1] There are in many animals two external coraco-brachials, one passing to the internal condyle and lower part of the humerus, and the other inserted higher up, beneath the ulnar tubercle. The vessels pass between the two.

[2] This upper internal coracoid is described by Mr Mivart in his excellent paper on the Echidna (*Linn. Trans.* xxv. 385). I can bear witness to the correctness of his description of this and most of the other muscles of that animal. The coraco-brachial of the bird belongs to this internal set, whereas the so-called *pectoralis tertius*, shewn by Prof. Rolleston to be a coraco-brachial, belongs to the external set.

EXPLANATION OF PLATE III.

Fig. 1. The outer surface of the pelvis of an Echidna (natural size): 2. the same of a Beaver (one half of the natural size): 3. the same of a Hare (natural size): 4. the same of Pteropus. In all these *il.* is the surface for the iliacus muscle, and *gl.* that for the gluteal muscles.

Fig. 5. The outer surface of the scapula of a Horse (one-fifth the natural size); *s. sp.* the surface for the supra-spinatus muscle; *i. sp.* the surface for the infra-spinatus muscle.

Fig. 6. The outer surface of one side of the shoulder girdle of a Chameleon (nat. size).

Fig. 7. The outer surface of one side of the pelvic girdle of a Chameleon.

THE ACTION OF THE HORSE. By NEVILLE GOODMAN, M. A., *St Peter's College, Cambridge.*

MY only reason for making any criticism (Vol. IV. p. 8. of *this Journal*), on Mr Gamgee's remarks (in Vol. III. p. 370) on the action of the horse, was that I found in an article which professed to correct previous errors, a statement which my previous knowledge, however small, enabled me to recognize as incorrect.

Mr Gamgee, in his last article (Vol. IV. p. 235), has so entirely evaded all reference to his own error and my refutation of it as to leave the impression that, while he has been too keenly aware that I made some criticism upon his article, he is quite ignorant of its nature. The sole result of that criticism, so far as he is concerned, is to produce the statement that "he cannot find a sentence that requires altering," and that "he holds to the strict accuracy of his paper." I am compelled, therefore, to reiterate that the sentence which I hold to be strictly *in*accurate, and which no amount of alteration, short of making it mean exactly the reverse of what it does mean, could make it approach the truth is the following (Vol. III. p. 371.): "The horse in the fast paces, as in the slowest movement, has never less than two of his feet acting on the ground." This statement I demonstrated to be false from Mr Gamgee's own diagram and accepting his own measurements. When therefore, Mr Gamgee makes this the occasion of stating that his "conclusions are based on observations and accurate measurements of the paces of hundreds of horses," he simply strengthens my proof of the incorrectness of this statement. I can only refer the readers of this *Journal* to my remarks on this point, and leave them to judge whether Mr Gamgee has left this position so totally undefended, because it is *impregnable* or *indefensible*. Surely too Mr Gamgee exhibits a misplaced sensitiveness, when he notices that I speak of *the horse* from which he took his measurement; for in looking back to his first article, I find that he himself in speaking of the diagram to which I referred, states that it is taken from the footing "of a two-year-old colt in training." I have no

wish to depreciate Mr Gamgee's experiments. If he has taken accurate measurements of the paces of hundreds of horses, I know how laborious the process, and am only surprised that such labours led him to form a theory which a few moments' thoughtful digestion would have shewn to be untenable.

Though Mr Gamgee has failed to meet, or even to notice my objections to what I conceive to be his error, I will not follow his example by evading his strictures on what he thinks to be mine. My statement, that "there is no essential difference between the canter, gallop, and racing pace" is looked upon as erroneous. Now what I meant by this, and what I think the words imply, is that a horse passes from the slowest prancing and curvetting through the paces usually called cantering, to the hand-gallop, and so on to the full gallop by insensible degrees, so that it is impossible to draw any fast line between these paces. Mr Gamgee states that the "characteristic of the canter which distinguishes it from all other paces, is the parallel position of the leading fore, and its diagonal hind foot at every third step." This definition is totally without meaning of any kind. The phrase "third step" is ambiguous, and the two feet mentioned are, of course, always parallel. As, however, the words "as shewn in the diagram" are added, we learn that what the writer intended to state is, that a horse canters when, in each stride or complete series of actions without repetition, he brings his left hind foot as far forward as the place on the ground once occupied by the right foot. The far more important question of when relatively, in time, these two feet occupied those points is entirely ignored. If the writer chooses thus to define the canter, of course he can do so. That a horse would be said to be cantering, and does really carry his rider easily both when the left fore leg falls short and oversteps this relative position, I am convinced. To establish my own so called *error*, I have obtained accurate measurements of the tracks of a horse when passing through the paces above enumerated. During this operation the measurements show that the horse passes through exactly the position defined by Mr Gamgee as *the canter*, but I am so convinced that this definition is an arbitrary convention, that I would put the horse through the same series

of actions, and defy any one either on or off the horse to detect at what moment the horse is cantering according to Mr Gamgee's definition.

Holding as I do to the truth of my own statement about the canter, I am precluded from giving a strict definition of this pace; but, as easy carriage is the desideratum of the canter, I should say that this is best accomplished when the right hind foot and the left fore foot come to the ground at the same moment after the bound, in which all four legs have been lifted from the earth. According to this definition young foals canter, and so do sheep and pigs and other quadrupeds which I have closely observed. Mr Gamgee is not very explicit when he states that the canter of the horse differs as much from the gallop "as dancing does from walking and running in man." If he means that cantering is a purely artificial pace, he is certainly wrong.

In conjunction with my brother, Mr Albert Goodman, I have collected some facts, and made some deductions from these concerning the paces of the horse; but as we intend to put our theories to the test of constructing somewhat elaborate Zootropes according to these theories, and then seeing whether the judgment of the eye confirms the inferences drawn, I shall leave all further remarks on this interesting question until these are completed.

ON THE ACTION OF NITRITE OF AMYL ON THE CIRCULATION. By T. LAUDER BRUNTON, M.D., D. Sc.

THE property of causing flushing of the face, and throbbing of the carotids which nitrite of amyl possesses, was first observed by Guthrie in 1859, but no farther notice of it was taken till Dr Richardson in 1866, again drew attention to it. His experiments led him to conclude that it paralyzed the nerves from the periphery to the centre, lessened the contractility of the muscles and dilated the capillaries in the web of the frog. They were shortly afterwards repeated by Drs Gamgee and Rutherford, who however, found no action on the nerves, either sensory or motor, and rarely any on the capillaries of the frog. In some other experiments, also unpublished but whose result they have kindly communicated to me, they found that the sphygmographic tracing of the radial pulse undergoes a remarkable change, the waves becoming much more frequent and their ascent, but especially descent, much more rapid; and the pulse-rate and pressure in a manometer connected with the carotid of a rabbit fall when the vapour of the nitrite is inhaled. Previous division of the depressor nerves did not affect the result.

The diminished blood-pressure which it produces, led me to apply it in angina pectoris, and the good results I obtained made me anxious to investigate more closely the nature of its action. An excellent opportunity for doing so was afforded me by the kindness of Professor Ludwig, in whose laboratory at Leipzig, the experiments, the result of which I am about to give, were carried on. With the exception of one or two on dogs, they were made upon rabbits; and instead of allowing the animals simply to inhale the vapour, artificial respiration was employed, the apparatus being so arranged that the air could be either sent direct from the bellows, through a tube in the trachea, to the lungs, or passed through a vessel containing the vapour of the nitrite. The advantages of this arrangement were, that the bellows being worked by an engine with great regularity, the disturbing influences of unequal respiration on

the blood-pressure were to a great extent avoided. One of the chief of these is that any strongly-smelling vapour, and nitrite of amyl among others, acting on the nose of rabbits, causes suspension of the respiration for a short time; and the alteration in the condition of the blood thus produced causes irritation of the vagus and slowing of the heart's action; such as Drs Rutherford and Gamgee found accompanying the sinking of the blood-pressure in rabbits.

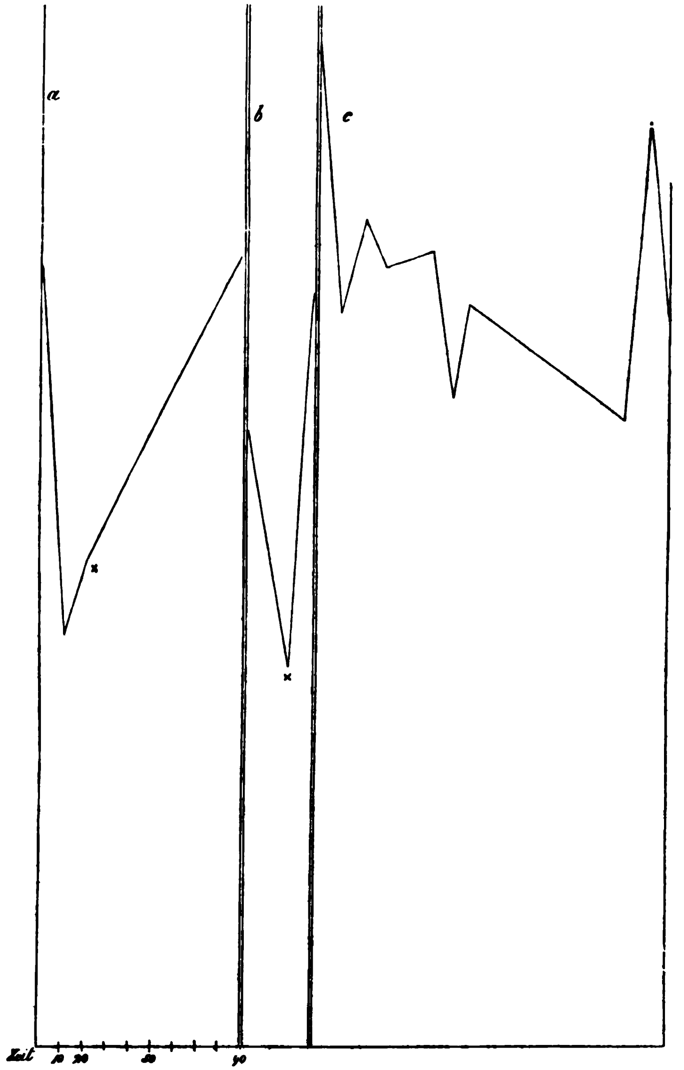
When air charged with the vapour was passed directly into the trachea of a rabbit the blood-pressure almost immediately sank very much, but the pulse-rate remained nearly unchanged. As the pressure sank general convulsions took place and the pressure immediately rose notwithstanding the continued inhalation of the vapour, the pulse curves becoming at the same time indistinct, so that the rate could not be well ascertained.

When the vapour was discontinued after 20 seconds the pressure rose still more quickly, and in a minute at most attained its normal height as is seen in Fig. 1. where the distance along the abscissa indicates the time and the ordinate, the pressure in millimetres of mercury. This shows that very small quantities of the drug produce a great effect and that its action speedily passes off, the vapour being either excreted or destroyed in the body.

In order to avoid the disturbance occasioned by the convulsions, the animals were then poisoned by curare and the vapour administered. The pressure, as before, sank immediately and did not return to the normal amount so long as the inhalation was continued. It did not however sink constantly and then remain at a definite minimum, but oscillated up and down just as Traube observed it in curarized animals, and as is shown in the last two curves of Fig. 1.

It is possible that the convulsions which occur readily in rabbits, but which I have only once, and that to a very slight extent seen in man, are suffocative, like those produced by CO, for Dr Gamgee has shown that nitrites acting on the blood prevent hamoglobin from giving up its O. This is the more probable as the respiration is first affected and if a drop of nitrite of amyl be mixed with water and the vapour thus diluted be administered, the limbs remain quiet but the ani-

Pressure in mm. Hg. Fig. 1.



Not poisoned. Poisoned with curare. Seconds.
* Inhalation stopped.

mal begins to make respiratory movements independently of the bellows, and when the vapour is less diluted these become more and more marked till general convulsions take place.

The diminished blood-pressure might be due either to a lessened power of the heart, or a dilatation of the arteries and a consequently diminished resistance. That the latter is the true cause, is rendered probable by the flushing which the vapour causes, both in the human face, and the rabbit's ear, and is shown by what might at first seem an anomalous action in some dogs. When the pulse in dogs is slow, the inhalation of amyl produces comparatively little effect on the blood-pressure; and it might be thought that its action was different in them from rabbits, but the reason is that the pulse, which in rabbits is naturally rapid, and remains unchanged by the outside, becomes in these dogs, remarkably quick almost as much so as in rabbits. If the vagi be first divided, so that the pulse in the dog becomes quick like that of the rabbit, and the nitrite be then inhaled, the pressure falls just as in rabbits. In order to confirm this view, and at the same time to decide the question, whether the dilatation of the vessels was due to a direct action of the substance upon them or to a diminution in the tone which the vaso-motor nerves derive from the medulla oblongata, another series of experiments was undertaken. This question was the more interesting from its connection with another research which I began under Professor Ludwig's direction, but unfortunately have not yet finished. Professor Ludwig observed, and directed my attention to the fact, that the alteration in the lumen of arteries noticed by Schiff, in the rabbit's ear, may be seen also in all exposed arterial twigs in the skin and connective tissue. They vary in amount and rapidity in different animals, and in the same animal at different times. They are sometimes absent, but in such cases may be generally produced by poisoning with curare, or by suspending the respiration, and when once aroused, they continue some time although the respiration be afterwards most carefully performed.

That these alterations are, at least in part, completely independent of the vaso-motor nerves in the brain is shown by their occurrence in the ear and other parts, after all the nerves sympathetic and cerebrospinal going to the part have been divided, and after division of the cord in the neck notwithstanding the low pressure which then remains.

The form of the variation, shows that they do not depend

on varying blood-pressure in the large arteries for sometimes a contraction suddenly appears between two parts of the artery filled with blood, and in one case in the rabbit's ear I noticed such a contraction take place in a small artery at the point where it branched off from a larger one, and proceed peristaltically downwards.

The lightest touch on an artery after division of the nerves, causes a movement generally limited to the part, and consisting not in a contraction, but in dilatation which remains for some time, and gradually disappears. As Gunning and Cohnheim have made similar observations on the tongue and web of the frog, and some facts in Sadler's research (see on Physiology) can only be explained by an independent motion of the vascular walls, it seems to be a widely extended and therefore important phenomenon. If the nitrite acts through the vaso-motor centres in the brain, it should have no effect if these be separated from the vessels by dividing the cord in the neck, but if its action be exerted directly on the vessels, the division of the cord will not prevent it, and that it in fact does not do so, will be seen from Figs. 2 and 3. The blood-pressure, which had sunk very low after division of the cord, sank yet farther when the nitrite was inhaled. Although the sinking was not absolutely great, it was so relatively to the very low pressure already existing and was analogous to that observed on the section of a second splanchnic nerve after division of the first. The other experiments to prove that the diminished blood-pressure after inhalation, is due to dilation of the vessels, and not to a weakened heart, consisted in compressing the aorta below the diaphragm and then administering the nitrite. If the diminution in pressure was due to a weakened heart, the inhalation of the nitrite should at once cause a diminution in the pressure to which the blood had attained after compression of the aorta. As only the circulation in the lower part of the body was in this way cut off, we cannot expect that no sinking should take place, but only that it should be less than the normal. In order to diminish the error from this source, these experiments were made after previous division of the cord in the neck so that the vessels should become relaxed, and the difference produced in their calibre by the vapour being thus less.

Fig. 2.

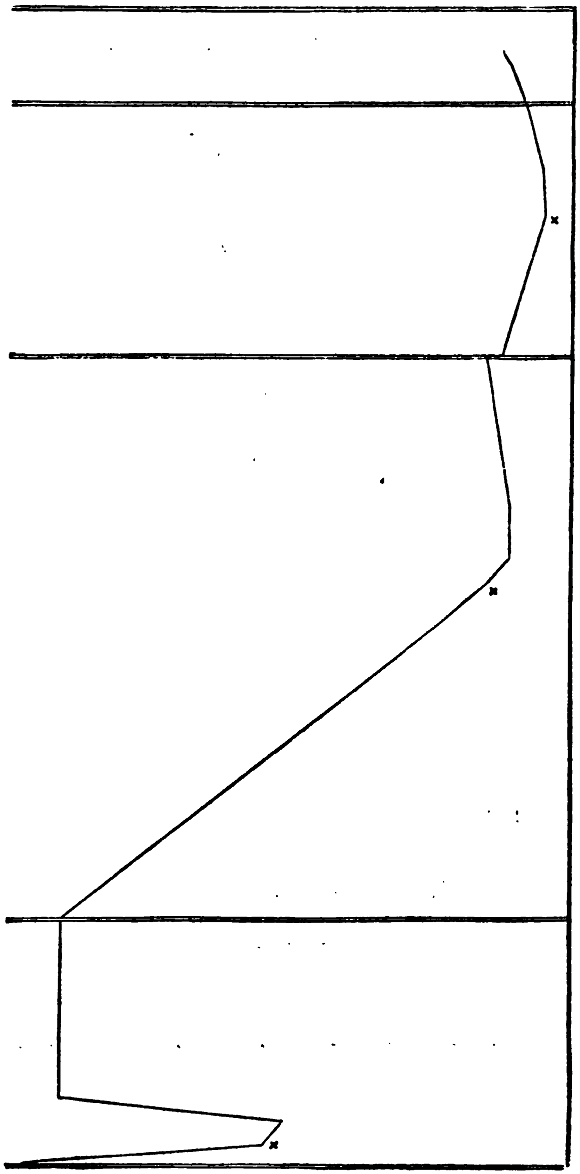
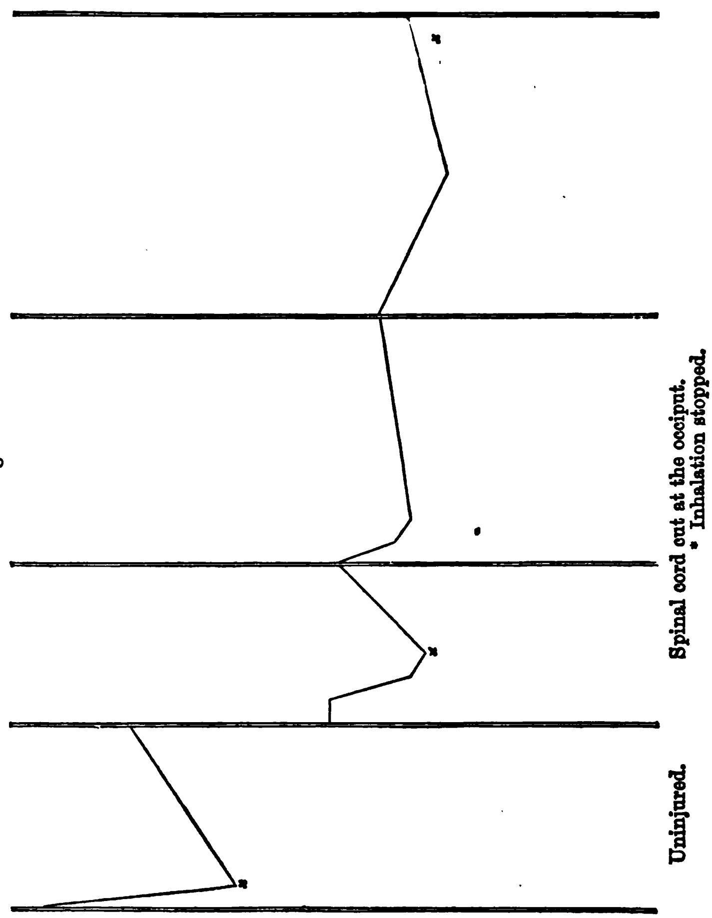

Uninjured.
Spinal cord destroyed at the axis.
* Inhalation stopped.

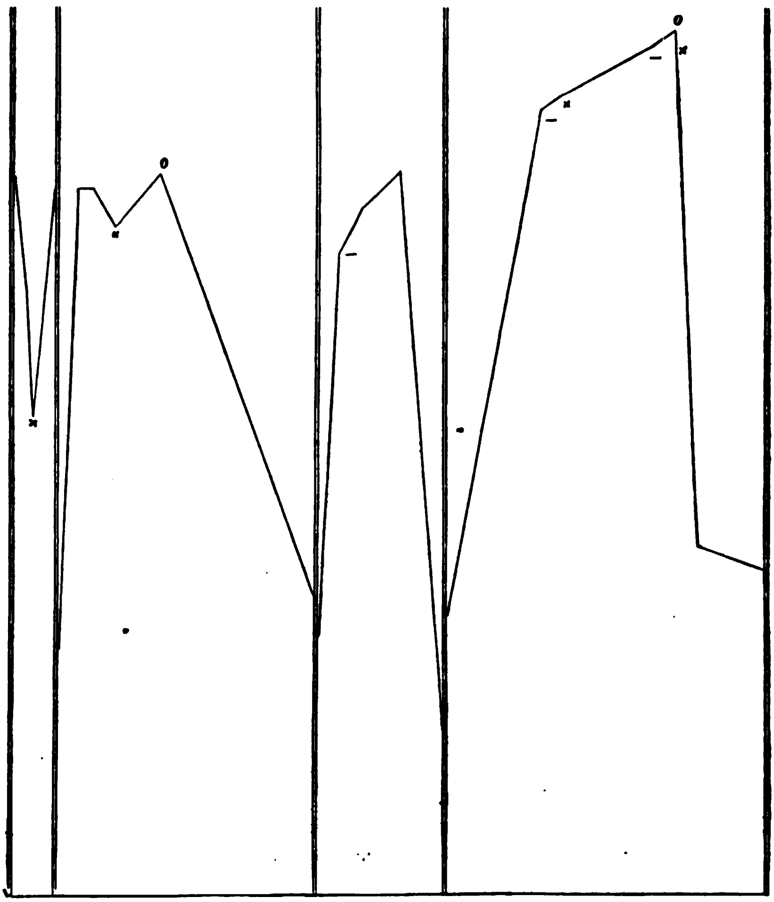
Fig. 3.



The results obtained were as shown in figs 4, 5 and 6, that sometimes a rise took place during the inhalation, but generally a sinking, much less however than in the normal condition.

We may therefore conclude that the diminution in the blood-pressure is not due to weakening of the heart's action, but to a dilatation of the vessels, and that this depends on the ac-

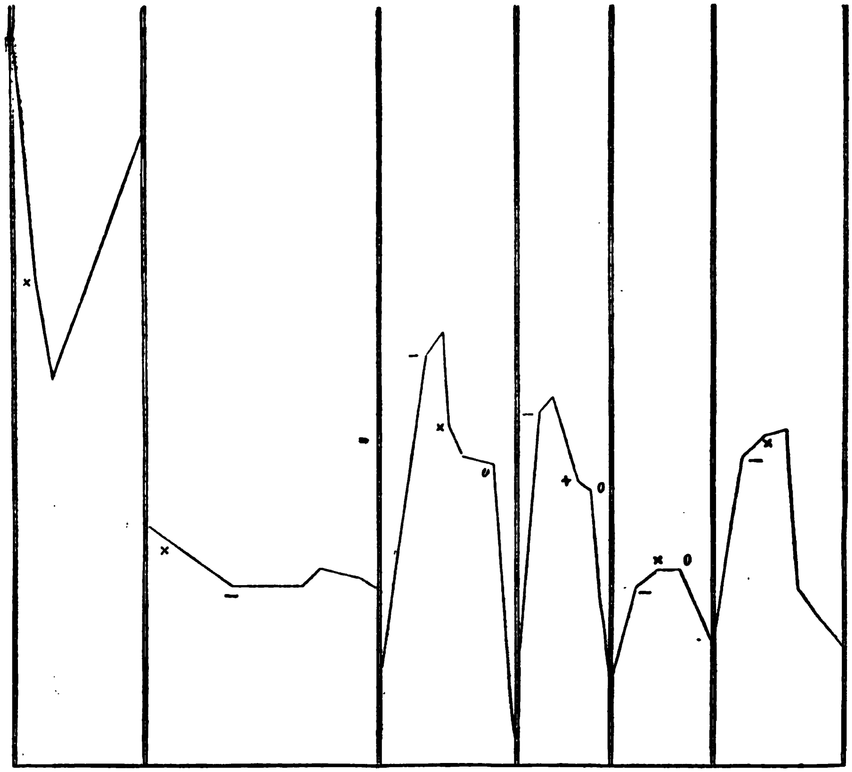
Fig. 4.



← Inhalation begun. * Inhalation stopped; at the beginning of the last three observations the Aorta was compressed. *o* Aorta opened.

tion of the nitrite on the walls of the vessels themselves. Whether this is due to its action on the muscular walls themselves, or the nerve-ends in them, cannot at present be with certainty said; and further experiments must be made to determine whether the walls of the arteries are the only structures consisting of unstriped muscle which are affected by it.

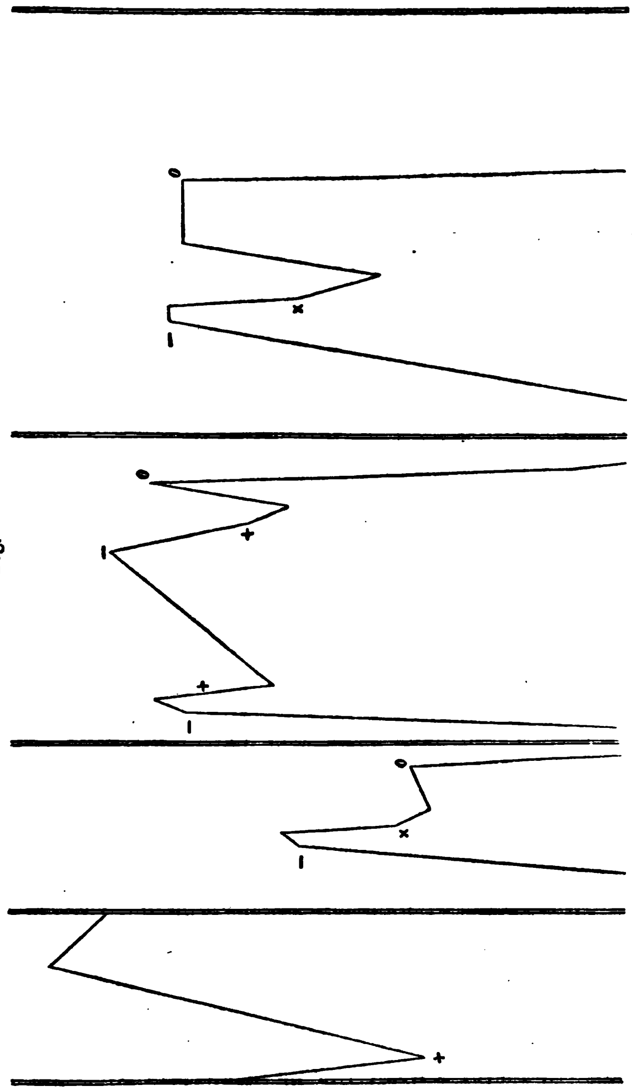
Fig. 5.



Cord cut in the neck.
In the last four observations the aorta was compressed; at *o* the aorta was opened.
− Inhalation begun. + Inhalation stopped.

The further dilatation which takes place after the usual tone of the vessels has been destroyed by division of the cord, seems to indicate that it is of an active nature analogous to that in the vessels of the penis after irritation of its nerves; and this would point rather to an affection of the nerves than of the muscular fibres. In conclusion, I desire to express my warmest thanks to Professor Ludwig for the great kindness he has shown me, and for his invaluable advice and assistance in this investigation.

Fig. 6.



The ordinates indicating the pressure, begin 50mm. above the abscissa. In the last three observations the spinal cord was divided; the aorta compressed, at o the aorta was opened. – Inhalation begun. + Inhalation stopped.

THE PHYSICAL RELATIONS OF CONSCIOUSNESS AND THE SEAT OF SENSATION, A THEORY PROPOSED. By Professor Cleland, Galway.

[Read at the British Association Meeting at Liverpool.]

In venturing to disturb the theory of Sensation as it has long been taught, I am very sensible that it may be difficult to obtain a patient hearing, seeing that it is a theory universally received; yet there are important points which that theory leaves unexplained, points familiar to all, and which only require to be mentioned for every one to admit, that as the theory at present stands, they are totally inexplicable. That theory may be shortly stated thus:—that an irritation applied in the neighbourhood of a nerve-extremity produces an impression which is conducted along the nerve till it reaches the seat of consciousness in the brain; and that the mind, affected by the impression, becomes thereby cognisant of a sensation, which it refers to the extremity of the nerve along which the impression has been conducted. Further, let it be recollected that nervous impression is nothing but a physical condition, some of the peculiarities of which have been laid bare by experimenters, and which is capable of affecting any of the nervous elements, viz. nerves, both motor and sensory, and nerve corpuscles.

In speaking of nervous impression thus defined, we deal with a matter of fact, although we are not thoroughly acquainted with its details; but in stating the doctrine of the modus operandi of sensation we have merely to do with a theory.

This theory is, however, a physiological as well as psychological theory, and involves the consideration of the functions of nervous structures, as well as the laws to which consciousness is subject; and I am all the more anxious to point this out, because the physiologist is liable to think that where consciousness is involved physiology cannot be concerned, whereas the doctrine of sensation, although it relates to a matter on the psychological frontier, is arrived at from physiological data—and it is because it is, as I hope to shew, at variance with

other anatomical and physiological data that I find fault with it.

It may be taken as certain that when a nervous impression is conducted to the seat of consciousness in the cerebral hemispheres, corpuscles there enter into a corresponding condition to that of the conducting nerve, precisely as a corpuscle which is the turning-point in a reflex action is no doubt affected by the condition which it receives from the sensory and passes on to the motor nerve. But if we imagine that in an act of consciousness the corpuscles of the hemispheres undergo any change other than that of passing into the impressed condition studied in nerves, we become guilty of an assumption which has the plain objection of being unfounded. That assumption is unfortunately often made, apparently from confusion of ideas; for authors, particularly medical authors, speak as if mental impressions lodged in the brain; whereas nervous impression, the only active condition into which there is evidence of the brain passing, is a physical state of a uniform nature, while, on the other hand, a mental impression is the presence of a notion in the mind, and the variety of such notions is infinite. I shall return to the consideration of the bearing of this remark on views as to the details of the functions of the hemispheres, but wish first to direct attention to the difficulties of the received doctrine of sensation.

I own that, even supposing all difficulties as to the route pursued by impressions from the periphery to the brain to be removed, to me it is utterly inconceivable that the sites of irritations over the whole surface of the body should be minutely indicated, and a more vague appreciation of the positions of internal irritations be obtained, by differences in the cerebral termini of the impressions conveyed from different parts. The supposition involves the difficulty that there is no mode by which the mind of the child could ever learn to associate the changes taking place at the cerebral termini with changes at different parts of the surface. If the consciousness were ignorant of the surface of the body at the commencement of life, it must continue always to be so, for want of means from which to derive the information; no amount of custom could avail it, for the mind gets all its experience from the

senses, and until it could refer sensations to the parts of the body from which they were derived, it could gain no experience of the external world whatever. Therefore the doctrine of sensation necessitates the assumption that the functional union of the parts of the periphery with different termini in the brain is primordial, and that the surface of the body is minutely represented or repeated by a number of points in the brain, which, however confusedly massed together, derive their properties from their connections.

This assumption has possibly never been stated in this form before, but it is one from which the received doctrine of sensation allows no escape, and surely it is an assumption sufficient to arouse some suspicion of the doctrine which demands it. If it be true, it seems strange that some of those termini are not often the seat of disease while others escape, that we do not meet with paralysis of the sensation of limited patches of the body occurring from limited cerebral lesions, and that nothing of the sort has been laid bare by the experiments of vivisectors.

If we now contemplate the routes by which impressions from the periphery reach the brain we meet with other difficulties. It is plain that if the mind is affected by the condition of cerebral elements as if they were situated at the parts from which they receive impressions, then the accuracy of the mind's knowledge of the periphery is dependent on the number of such elements which receive impressions from distinct parts, and that each distinctly recognisable spot of the body must be joined by a separate tract with its own cerebral terminus. That tract may be interrupted by corpuscles with branches in other directions, and must be so to account for reflex actions, but it cannot, in any part of its course, pass through a fibre common to it and another spot of the periphery, without the consciousness being bereft of all means of distinguishing the one spot from the other. But all that we know of the structure of the cord notoriously contradicts the notion of such an arrangement, —an arrangement which would involve the continual accumulation of additional sensory tracts in the cord from its lower to its upper extremity. Moreover, it appears certain from the experiments of Dr Brown Sequard that the greater part of the conducting material of the cord, its white matter, may be

divided, and, provided the grey matter is left intact, sensation in the parts beyond the lesion remains unimpaired. Thus, the routes of communication between the brain and nearly the whole surface of the body are known to pass through the extremely limited area exhibited by a transverse section of the grey matter of the cord, and probably more than half of that area is occupied with reticulum which has nothing to do with nervous conduction, while of the nerve-fibres which traverse it, a number are motor, and according to the theory some must be devoted entirely to impressions from internal parts, even though such impressions are principally pathological. Can it be believed that the remaining fibres traversing such a section in numbers approach what would be necessary in allowing one nerve-fibre for the area of distribution of each nerve-fibre on the surface of the body?

If it be attempted to escape from this difficulty by attributing with Dr Beale a function of importance to those numerous striæ or fibrillæ which that writer, and after him, Max Schultze, have described in nerve-corpuscles and axis-cylinders, the importance of a single nerve-fibre as a conductor may be indefinitely multiplied, but the assumption is made that those fibrillæ are really present during life, and that each one is capable of maintaining the active or the passive condition quite independent of the others with which it is in close alliance; an assumption which must appear sufficiently great to those who remember how little individuality belongs to the much more easily demonstrated fibrillæ of muscular fibre. Even then, the difficulty remains as much as ever; for, if we suppose that those fibrillæ are continued down all the poles, and that the peripheral nerves possess them, as well as others, we have indefinitely multiplied the tracts in the periphery as well as in the cord, and left the disproportion in numbers the same as ever; while, if we deny fibrillæ to the peripheral nerves, and suppose them to exist in the fibres of the cord alone, we assume that the work which requires a whole fibre in a peripheral nerve, can be done by one of a multitude of striæ in a fibre of the cord, which seems improbable.

If the difficulties which have been enumerated appear great, when the received theory is applied to the explanation of com-

mon sensation, they are still more striking when it is applied to vision. The theory demands that for every ray of light appreciated by the mind there must be a completely distinct communication from an element of Jacob's membrane to a terminus in the brain, and that the impressed condition of every such terminus must be capable of creating in the mind a knowledge of the position of the point on which the ray falls as related to all the other impressed points of the retina; and that supposition involves all the difficulty which has been pointed out in the case of common sensation; while the anatomy of the retina, even more distinctly than that of the spinal cord, contradicts the possibility of distinct communication between each of the immense number of peripheral nerve-terminations and the seat of consciousness. Within the retina, the threads leading from the elements of Jacob's membrane already enter into a complicated ganglion most developed in the part where vision is clearest, and it would be very hard to imagine that the fibres of the optic nerve emerging from the ganglionic corpuscles correspond individually with distinct rods or cones.

These being the objections to the received theory of sensation, they appear to me to warrant search for an escape from them, and I have been led, by the consideration of the properties of the living corpuscles of the body, and of what appear to me the established facts with regard to the actions of the cerebral hemispheres, to a hypothesis which I venture to put forward, believing it to furnish that escape, and to be in harmony with all that is known of the nervous system.

But before doing so I find it necessary to explain and defend the view which I hold with regard to the connexion between mind and brain, and which I have already in a few words suggested in a paper on the structure of the cerebral convolutions in the *Quarterly Journal of Microscopical Science* (April, 1870). We must inquire into the relations of consciousness with the hemispheres, before we ask how it is brought into communication with the finger-ends.

The cerebral hemispheres, which are well ascertained to be the organs without which none of at least the higher acts of consciousness can be performed, are a pair of highly corrugated vesicles, closely connected one with the other, and continuous

with the rest of the brain by the lower margins of the inlets into the cavities of the vesicles, where are situated the corpora striata. The structure throughout the different lobes and convolutions of the hemispheres, notwithstanding minor differences, is fundamentally the same. This grey matter consists of strata of corpuscles and nucleated protoplasm, and the vast majority of the corpuscles are connected with fibres ascending from the corpora striata and with others which lie horizontally, that is, parallel to the surface, while they send another stronger process perpendicularly outwards. The horizontal fibres exist throughout the thickness of the grey matter, but are massed particularly in a varying number of strata, while others of them coat the surface in various places. We have thus an arrangement fitted to allow consentaneous action of the whole grey matter of the hemisphere, but nothing pointing to different functions of the different regions. The convolutions are accounted for when it is considered that they bring the vascular supply within easy reach of every part of the grey matter, and the minor differences of structure may reasonably be supposed to arise from the different relations in which the different parts of the hemisphere lie to the corpora striata, and from unequal development of the nerve-corpuscles; a supposition in favour of which is the circumstance that in the posterior lobes, the parts furthest from the corpora striata, the nerve-corpuscles are least developed, and the horizontal fibres are most abundant.

But not only does the study of structure thus point to the probability of the whole hemispheres having one combined function, but development, comparative anatomy, and experiment point to the propriety of considering the corpora striata as forming with the hemispheres one organ. The corpora striata are enlargements of the basal portions of the hemisphere-vesicles; so says development: they are inseparable from the islands of Reil; so says adult structure. They may be irritated or damaged by vivisection in mammals, without damage to sensation or motion, and slicing them away in birds produces effects corresponding with those produced by slicing away the hemispheres in mammals; that is the evidence of experiment; while comparative anatomy shews the hemisphere-vesicles forming each a unity in fishes, a small distinct corpus striatum within

each in the turtle, and in the bird the whole hemisphere-vesicle converted into corpus striatum (or, more correctly, corpus striatum and island of Reil), with the exception of little more than a membrane at the upper part. The hemisphere-vesicle, therefore, appears to be a single organ primarily divisible, as Reichart has beautifully shewn, into a root-part which includes corpus striatum and island of Reil, and the 'mantle' which includes the remainder; and I apprehend that *the mantle is only a multiplier of the function of the root-part.* Let me revert now to one of the propositions made at starting, and remind you that while there is every reason to believe that the corpuscles of the hemispheres pass into the impressed condition studied in nerves, there is no vestige of evidence that they have any additional active condition, and you will, I trust, see that the plain explanation of the functions of the hemispheres is that they are so connected with the mind that *the total amount of mental action at one time is dependent on the total amount in the hemispheres of that physical state which we call the impressed condition.* Consciousness and the impressed condition of brain-substance go always together, but that impressed condition of the brain-substance is of one invariable nature, while the objects which may occupy the consciousness, its acts of memory, of observation, of reason, of volition, and its conditions of emotion or desire are of endless variety. The hemispheres, including the corpus striatum, may therefore not inappropriately be termed the organ of attention, the mode of whose action may be described thus:—the total amount of attention at any one moment is in proportion to the total amount of the impressed condition of the whole corpuscles of the hemispheres; but that attention may be occupied with numerous different mental actions going on at the same time; and with the specific nature of its occupation, whether memory, reason, emotion, appetite or volition, the brain can have nothing to do.

This view stands in direct opposition to the views which find favour at present with at least many physicians of mental disease. Starting with the proposition that the diseases of the mind are the diseases of the brain, which has a certain amount of truth in it, they have unconsciously slid from that starting-

point into phrenological assumptions and into a belief that brain corpuscles are pigeon-holes of ideas; and thus they predicate of matter properties incomprehensible and of which there is no evidence. But it is remarkable that neither medicine nor surgery, nor experiments on animals, have shown any difference of function in the different parts of the hemispheres. Wounds of the brain and limited spots of disease do not vary in their symptoms according to the part of the roof of the hemisphere attacked. I am not forgetful of the remarkable phenomena of aphasia nor the discoveries of Broca, but I have pointed out elsewhere* that the connexion between aphasia and lesion of the brain external to the island of Reil is best explained, not by reversion to the doctrine of the divisibility of the hemispheres into different organs, but by taking into account the variety of processes required to go on consentaneously to produce speech, and considering the interruption to the consentaneous action of the hemispheres by interference with the fibres of communication at the most central part of the organ. It is not only in the case of speech but in all the more complicated operations of thought that the mind has to attend to different matters at the same time, and it can well be imagined that simultaneous and harmonious action of the brain-elements is very necessary to secure that end. Thus it will be seen that the doctrine of brain-action which I have proposed by no means separates altogether the character of the mind from dependence on that of the brain. There are three elements on which the character of the brain as affecting the mind may be safely presumed to depend, viz. the intensity and ease of action possible to individual brain-elements, the total amount of those elements, and, as it appears to me, most important of all, the freedom and perfection of communication of those elements one with another; and it may well be supposed that this last requisite is most liable to be deficient in large brains, as the distances are greatest and the elements to be joined together most numerous in them.

In the structure of the brain there is the closest affinity to the structure of the rest of the nervous system, and its corpuscles are elements plainly comparable with the living parts or protoplasm-derivatives of other textures, and there is ground to

* *Medical Press and Circular*, 11 March, 1870.

presume that in like manner the impressed condition of nerve-corpuscles, whether in the brain or elsewhere, and of nerve-fibres, is analogous to the contraction of muscular fibres and of amæboid corpuscles: the peculiarity of those of the hemispheres being simply that action of the mind excites their action, and that their action excites that of the mind. That presumption prepares the way for the hypothesis of sensation which I venture to suggest, which is this: that *the consciousness extends from its special seat so far as there is continuity of the impressed condition;* that when an irritation is applied to a nerve-extremity in a finger or elsewhere, the impression (or rather impressed condition) travels as is generally understood, but exists for at least a moment along the whole length of the nerve, and that as soon as there is continuity of the impressed condition from finger to brain the consciousness is in connexion with the nerve and is directly aware of the irritation at the nerve-extremity. Or the position may be shortly stated thus: functional continuity between nerve-extremity and brain is proved to be necessary for sensation, while on the other hand existence of distinct routes of communication between them is highly improbable; and seeing that functional continuity is sufficient of itself to explain the phenomena, we are not entitled to assume the existence of distinct routes, as has hitherto been done.

Let it be distinctly understood that I do not say that consciousness resides in the nerve-extremities, but only that when the nerves are in the active or impressed condition in their whole extent up to the brain, the consciousness is affected directly by the irritations applied to their extremities. The relation then of consciousness to the brain remains totally different from its relation to the nerve. The impressed condition of the cerebral corpuscles produces only acuteness of intelligence: the impressed condition of a peripheral nerve or its extremity continued up to the brain brings the consciousness into communication with the irritation applied.

At the extremities of the different nerves are placed various arrangements which are least complicated in the nerves of general sense, and which modify their capability of being irritated by different stimuli. Thus the expansion of the optic

nerve is incapable of being affected by the irritation of light, but the elements of Jacob's membrane are irritated by them with ease. The complicated nerve-extremities of the ear are acted on by sounds, and the comparatively simple extremities of the nerves of general sense are acted on by mechanical irritations and changes of temperature. It appears, then, that if we suppose that the consciousness is directly affected by the application of the irritation to the nerve-extremity, we have the simplest possible explanation of differences of colour and of notes, and of the different kinds of sensation, conveyed by the nerves of general sense. We are in a position to say that as the irritation varies so varies the sensation, without being obliged to assume different kinds or conditions of nerves without evidence.

The objections may be urged against my theory that irritation of the ulnar nerve produces sensation in the finger ends, that persons after amputation of an arm feel pain in the fingers, and that irritation of one nerve often causes pain in another, as when disease of the hip gives pain in the knee, or toothache pain in the temple; but it appears to me that all these things are better explained by this theory than by the received one. When one strikes the ulnar nerve at the elbow accidentally with force every body knows that the acute pain is not felt in the fingers, but at the part struck, and that this is immediately succeeded by pain and a peculiar sense of vibration which travels downwards from the struck part till they reach the fingers. A patient who has had a limb removed, in like manner, is perfectly aware of the site of irritation when a nerve-trunk in the stump is touched, and it seems reasonable to suppose therefore that the sense of the presence of the removed part is somehow to be explained by habit. The production of pain in the temple from toothache may be explained on the supposition that intensity of the impressed condition sufficient to produce pain is conducted to a nerve-centre and spread centrifugally, just as I have suggested that the alteration in an ulnar nerve struck at the elbow spreads down to the fingers.

If I have put forward these views of sensation simply as a suggestion I should be still more unwilling to be dogmatic in applying them to the motor nerves. Yet it may be mentioned

that one of the most inscrutable of all phenomena, looked at from the received point of view, is the circumstance that the will is able to regulate delicately the movements of the limbs by adjusting the contractions of complicated muscles of which the mind is wholly unconscious; and the difficulty in this matter will be greatly simplified if we can see our way to believe that, when a motor nerve is set in action from the brain, the mind is brought into connexion with its distribution. Further, the hypothesis facilitates the explanation of muscular sense, and accounts for what has been observed by Dr Brown Sequard, that the tracts for muscular sense do not decussate in the spinal cord. The phenomena sought to be explained by the hypothesis of muscular sense are the consciousness of the position of a part, the capability of regulating its movements even when ordinary sensation is paralyzed, and the consciousness of the maintenance of muscular effort in such parts. The hypothesis which I offer in explanation of these phenomena is that by continuity of the impressed condition from the brain to the distribution of the motor nerves we are conscious of the parts to which the distribution extends, and of the exercise of the will within them. But, it will be alleged in reply, when we move our fingers we feel the movement not in the muscles of the forearm, but in the fingers. I think however that this difficulty will not appear so great when we analyze our sensations in endeavouring to make some unaccustomed movement. Thus, although in moving our limbs we are not conscious of any sense of effort which we could localize in the region of the muscles brought into action, unless perhaps when unaccustomed resistance is met with, it is different when one attempts to move the pinna of the ear or bring the palmaris brevis into action. If I attempt either of those actions I am conscious of exercising an effort to produce contraction in the neighbourhood of the part, on that side of it towards which I wish to move it. It seems to me therefore that muscular sense, as I have sought to explain it, is sufficient, in conjunction with experience, to account even for the movements of the fingers.

The bearing of the hypothesis now put forward on the functions of the nerves may be expressed in a few propositions.

1. The irritation of a nerve of common sensation throws

the nerve into the impressed condition, and as soon as that condition is continued to the brain, the mind recognises the irritation at the site where it is applied, in the form of sense of touch, temperature, or pain, according to its character. Over-intensity of the impressed condition may also itself be recognised in the form of pain.

2. Nerves of special sense differ from those of common sensation both in the circumstance that the apparatus at their extremities is affected by irritations of a different kind from those which affect other nerves, and in their irritation being recognised in the form of the special sense to which they are devoted.

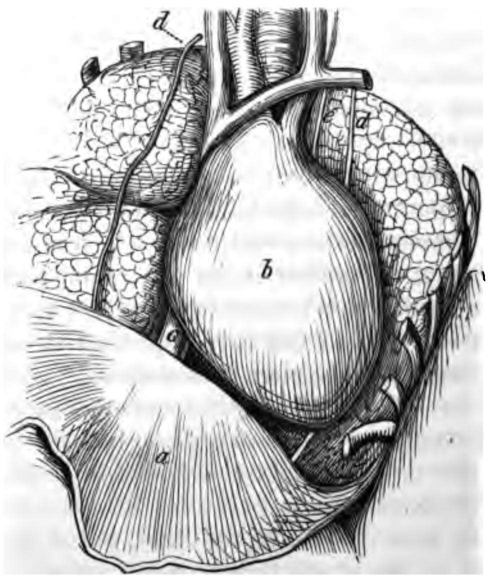
3. By the impressed condition continued from the brain to the distribution of a motor nerve not only is a stimulus communicated to the muscles and applied by the nerve, but muscular sense is given; and, the consciousness being brought into direct communication with the part, the will is enabled to regulate the position of the part and the degree of muscular energy with which it is maintained. But a motor nerve differs from sensory nerves of all sorts in the fact that irritation of it does not produce any sensation either of the character of common sensation or special sense; and in this respect it is like the proper fibres of the spinal cord and brain.

It may be allowable to say in conclusion that in publishing these views I put most importance on the objections which I have urged against the received doctrine of sensation; and in venturing to suggest another in its place I am perfectly aware that there is much which is imperfect in the hypothesis which I have put forward, and I can only hope that it may prove of some use in building up a more perfect view.

CASE IN WHICH IN MAN THE PERICARDIUM WAS UNATTACHED TO THE DIAPHRAGM, WITH A PARALLEL ILLUSTRATION FROM THE WALRUS. By Professor Turner.

It is well known that the pericardium is intimately connected by its fibrous layer to the upper surface of the central tendon of the diaphragm, by which connection it is fixed and completed below, and that its serous lining covers a portion of the thoracic surface of that tendon.

In the month of November, 1869, an adult male subject was dissected in my practical anatomy rooms, which presented a remarkable departure from the usual arrangement. My attention was not called to the part until after the chest had been opened, the lungs turned on one side and the mediastinal pleuræ removed, so that I cannot state what the disposition of



a. The diaphragm. *b*. The pericardium. *c*. The inferior vena cava. *d. d.* The phrenic nerves. *e*. The left vagus.

From a drawing of the dissection by my pupil Mr James Foulis.

these portions of the membranes had been. I found that the pericardium formed a perfect bag below, that it simply rested on the diaphragm, and that there was no continuity of fibres between them. Hence the heart and pericardium were together freely movable from side to side, and could be drawn forwards as far as their connections to the great vessels at the base would admit. The diaphragm consequently was not in direct relation to the serous lining of the pericardium. The pericardium closely embraced the heart, and in its shape corresponded to the form of the included organ.

The inferior cava pierced the diaphragm in the usual position and pursued a course of one inch before it pierced the pericardium to end in the right auricle. The heart and great vessels were in other respects normal.

At the time when this subject was in the dissecting room I was engaged in the dissection of a calf walrus, about one month old. In that animal I observed that the pericardium had an arrangement closely similar to that just described in the adult male, and as I know of no account of the relations of the pericardium in the walrus, I shall briefly describe it. When the right pleura was traced from the sternum along the side of the pericardium, it in part extended along the root of the lung to the surface of that organ, but behind the root it was continued along the dorsal surface of the pericardium to its left border, where it came in contact with the left pleura, and from the dorsal surface of the pericardium the two pleuræ were reflected together to the œsophagus. Along the plane of reflection the two membranes were as completely in contact with each other as are the two descending layers of the great omentum in man, and, like the omentum, they were perforated with numerous holes, so that the two pleural chambers communicated with each other. The hand also could be readily passed from the right pleural cavity between the pericardium and œsophagus, which structures were not therefore tied together by connective tissue. The pleuræ were prolonged backwards from the sides of the pericardium to the anterior surface of the diaphragm, and constituted indeed the bonds of union between that bag and the muscle. For the fibrous bag of the pericardium was not continuous with the central tendon, but formed

by means of its own proper fibres a perfectly closed bag at its diaphragmatic end, which bag closely embraced the heart and corresponded in shape to that organ. As the right pleura passed from the pericardium to the diaphragm, it invested the inferior vena cava, which vein lay for about four fingers' breadth in the cavity of the thorax before it pierced the pericardium. A broad fold of the pleura passed from it to the hinder end of the pericardium. Owing to the want of any direct connection with both the œsophagus and diaphragm the heart and pericardium could be freely moved from side to side.

In the perfect condition of the pericardial bag, in the absence of any direct connection with the diaphragm, in the consequent mobility of the part, and in the presence of a very recognizable portion of the inferior cava in the thoracic cavity, the walrus possessed a series of structures parallel to those found in the male subject first described. In the walrus, in all probability, the arrangement is normal, in the man, as we well know, the arrangement is exceptional, so very exceptional indeed, that I know of no other case in which a similar disposition of parts has been described.

ON A RUDIMENT OF THE PANNICULUS CARNOSUS SUPERFICIAL TO THE TRAPEZIUS. BY PROFESSOR TURNER.

IN the first volume of this Journal, p. 252, I recorded several cases which had come under my notice, of rudiments of the panniculus carnosus occurring in the human subject.

In the month of May, 1867, a male subject dissected in my practical rooms presented an example, which differed in its position from those previously recorded, and which is of interest on account of its relations and the direction of its fibres. When the skin and superficial fascia were removed from the surface of the trapezius muscle on the left side, a slender muscle one quarter of an inch broad and three and a half inches long was exposed. It lay parallel to and about an inch from the spines

of the dorsal vertebræ. It was attached above by two distinct slips to the tendon of the trapezius, where it arose from the

a. The left trapezius. *b.* The rudiment of the panniculus.

spines of the 2nd and 3rd dorsal vertebræ, while by its lower end it was connected to the tendinous origin, from the 5th and 6th dorsal spines, of the same muscle. The trapezius arose no lower than the 8th dorsal spine.

In my paper in the 1st volume of this Journal, I argued that the musculus sternalis was to be regarded as a rudiment of the panniculus carnosus, situated superficial to the fibres of the great pectoral muscle, and not as a thoracic prolongation of the rectus abdominis. The little muscle which I have now described and figured is, I believe, also a rudiment of the panniculus, and from its position and the direction of its fibres I consider it to be, on the dorsal aspect, a muscle parallel in its arrangements to the musculus sternalis on the pectoral surface of the trunk.

ON RISSO'S GRAMPUS, *G. rissoanus* (Desm.[1]). BY JAMES MURIE, M.D., F.L.S., F.G.S., &c.; *formerly Pathologist to the Glasg. Roy. Infirmary; Assist. Conservator Roy. Coll. of Surg., Eng.; and late Prosector to the Zoological Soc. Lond.* (Plate V.)

1.—*Remarks on the Species, its Character, &c.*

IN a report[2] laid before the Parisian Academy of Sciences in 1812, by Cuvier, Lacépède and Geoffroy Saint-Hiliare, on some Cetaceans captured on the coasts of France in the beginning of that year, mention and a figure is given of a remarkable form from the Mediterranean. This Baron Cuvier denoted "le Dauphin de M. Risso," *Delphinus aries*? though believing it, from the drawing sent him, to be none other than his *D. griseus*.

The original observer, Prof. A. Risso of Nice, subsequently in 1826[3] gave the characters more in detail and refigured the animal. Other systematists admitted the species, though still some doubts clung to it. The worthy Laurillard, however, was fortunate in being present at the capture of a male and female in the bay of Saint Jean, near Nice, in 1829; and by a sketch made on the spot, and a fresh description, established the identity of the creature. As *Phocœna rissonnus*, M. Fred. Cuvier, in his *Cétacés*[4], supplemented the last-mentioned author's notes by some remarks of his own and a new figure. Dr Gray[5] in various publications has recognised the species, and Gervais[6], I believe, first figured the skull. Lately Van Beneden[7] and Gervais, in their great work on the whale group, have given excellent illustrations of the skeletal peculiarities of the closely-allied and almost identical forms *G. griseus* and *G. rissoanus*: but the

[1] Laurillard has been credited with the specific name *rissoanus*, but in searching authorities I find Desmarest in 1820 (*Encycl.* p. 519) seems first to have Latinized G. Cuvier's D. d. M. Risso thus. Other writers have used *rissoi*, *rissonnus*, *rissii*, &c. I prefer to retain the earlier name, especially as it is that in most frequent use.

[2] *Annales du Museum*, XIX. 1, Pl. 1.

[3] *Hist. Nat. de l'Europe Méridionale*, III. 23, Pl. 1, fig. 2.

[4] *De l'Histoire Naturelle des Cétacés*, Paris, 1836, p. 196, Pl. 13, fig. 1.

[5] *Zool. Erebus and Terror*, p. 32, and *Cat. B. M.* 1850, &c.

[6] *Zool. et Paléont. Franc.* p. 300, Pl. 37, figs. 1 and 2.

[7] *Ostéographie des Cétacés Vivant et Fossiles*, Pl. 54.

letter-press concerning them has not yet been issued. Such is all the original matter published concerning the species in question, although several writers refer to and even figure it; for which, and an epitome of the salient points, consult Gray's most useful *Cat. of Seals and Whales in the Brit. Mus.*, 1866.

It is rather curious that G. Cuvier's, Risso's, and Laurillard's (pub. by F. Cuvier) representations are widely different from one another, though they agree less or more in an essential particular, viz. irregular light linear devices slashed throughout the body. In fact these scratch-like lines are the exterior specific distinctive mark. Notwithstanding evident shortcomings in the figure, Risso's own simple description is very graphic: "Son corps est alongé, arrondi, renflé vers sa partie antérieure, diminuant insensiblement de grosseur vers la queue qui est déprimée; sa peau est mince, unie, de couleur grise à nuances bleuâtres, traversée par des traits irréguliers et des raies inégales, droites ou flexueuses, blanchâtres; le ventre est d'un blanc mat; la tête fort grande; le museau arrondi, releve en arc, obtus, percé vers la nuque par l'ouverture des évents; la bouche est ample, arquée; la mâchoire supérieure pourvue d'alvéole seulement, est plus avancée, et couvre l'inférieure, qui est garnie de chaque côte de cinq grosses dents coniques," &c. Laurillard states from his observations that the male and female differ in appearance, the former resembling Risso's description, but the latter of a uniform brown.

In the beginning of this year an adult female whale, readily identified with Risso's species by colour and scratch-markings, was brought to London, which, after being publicly exhibited for a short interval, was purchased by Mr Gerrard for the British Museum. Prof. Flower obtained Dr Gray's permission to investigate this rare form, and brought forward a notice of his intention to describe the skeleton in a future communication to the Zoological Society. Since then a second much younger female has been captured on our coast, which I had an opportunity of dissecting. As this disagreed in some ways from the other, I have thrown together my notes on its soft anatomy in the form of a communication; purposely avoiding all mention of its skeleton, which doubtless Prof. Flower, with his knowledge of the group, will skilfully interweave in his memoir.

Tables of admeasurements in inches and decimals, of the young female examined by me.

Extreme length, following the curve of the body .	79·0
From the snout to the fork of the tail . . .	75·0
Vertical depth of the body in front of the dorsal fin .	15·5
Distance between the anterior root of the dorsal fin and the snout, following the superficial curve . .	35·0
Taken in an approximate straight line the same points more apart, about	31·0
Distance from postero-margin of dorsal fin to fork of tail	33·0
Dorsal fin, length of its base	9·0
„ in height	6·0
„ its anterior marginal curve . . .	12·25
„ its posterior concave border . . .	7·0
„ breadth about its middle . . .	4·5
Distance from snout to root of flipper . . .	17·0
Pectoral limb, in extreme free length . . .	12·0
„ its greatest breadth . . .	3·5
„ diameter about middle of free portion .	2·0
„ girth at axillary end . . .	10·0
„ thickness about middle of flipper. .	0·6
Caudal fin, in extreme breadth	14·25
„ width base of each fluke . . .	5·5
„ ditto, about the middle of same . .	3·5

The shape of the body and fins and position of the latter, are shown at a glance in Pl. V., a faithful recopy of a careful drawing to scale made at the time. The colour of the entire back, fins and tail is of a neutral tint. The throat and abdomen to beyond the anus is white or with a tinge of yellow in it, a kind of streaked dirty white. This rises almost to the medio-lateral line and intermingled with faint but increasing streaks and mottles, and a shade of light brown, or rather purple tinge, graduates insensibly into the leaden or bluish grey of the upper half of the sides. There is a grey patch on each shoulder. The protuberant muzzle is the yellowish white; the caudal region behind the anus, dark. In truth with variation of figure, white forehead and mandible, it is uncommonly like a light-coloured porpoise. The grand feature of interest, however, is the

scratched-like lines which in this animal differed completely from hitherto figured specimens, or that examined by Mr Flower. Instead of innumerable obliquely intercrossing marks over the entire superficies and fins, the narrow white but grey-edged lines were seven in number, somewhat vertically placed along the sides from the shoulder to about opposite the vent; the front and hinder pairs being cross-barred longitudinally. So remarkable a feature, undoubtedly irregular in individuals, is notwithstanding a most prominent specific landmark. As to sexual coloration, I am not inclined to lay stress on Laurillard's statement of the female being brown as contradistinguished from the scale; in this case the brownish hue was most meagre. There is quite as great variety in the porpoise, and, so far as my observations extend, the male is occasionally the darkest, but this really may be a matter of age.

As regards the eye, this has the same narrow elliptical form common to the cetacea. From the anterior to posterior canthus or long diameter is 0·9 inch, and across or vertical depth 0·5 inch. It is placed ten inches posterior to the snout, two behind the angle of the mouth and one-and-a-half above it. The iris appeared of a deep purple tint; but whether this was a post-mortem effect or the natural hue, I will not pretend to say, as Risso notes it is golden-coloured.

I may mention that a double row, four in each, of bristle roots[1] existed on the upper lip. The caudal region is ridged above, stopping short of the tail fork, and there is a less notable keel beneath.

2.—*Generative parts and muscles around.*

The opening of the vulva, four inches long, is situate two feet in front of the fork of the tail; the measurement being taken to the posterior angle of the former. The anus lies a couple of inches behind. In the simple commissural lips of the generative outlet, development of clitoris, and lateral fissures of the mammary gland, there is no departure from the cetacean type. Neither did I observe any special deviation of the urogenital organs interiorly.

[1] Vide Rousseau, *Ann. d. Scien. Nat.* Nov. 1830.

In my memoir on the "Deductor" I have made some original investigations on the muscles of the pelvo-generative parts, hitherto little heeded in cetacean dissections. Having figured these, I refrain doing so in the Grampus, but make a memorandum of what I observed.

The fleshy rectus abdominus terminates in a wedge-shaped manner opposite the anterior pudendal angle and clitoris, and is continued by a tendon which spreads out in an aponeurosis between the vulva and rectum. Here it passes beneath the backward extension of the great panniculus carnosus, which extends triangularly along the medio-lateral line to about opposite the hinder angle of the uro-genital slit.

There is a well-developed internal and external sphincter ani. A broadish sheet representing a transversus perinæi reaches from the post-border of the rectum forwards to the vulva. I observed also, what I have incidentally noticed in globiceps, beneath the last and midway betwixt the rectum and vulva, a layer of transverse or somewhat oblique muscular fibres, half an inch in antero-posterior diameter, which passed from the median line across the ilio coccygeus. On the left side part of its fibres ran on to the inter-pelvic fascia.

A small but distinctly separate muscle is met with on the outer side at the anterior end of the generative outlet, and divergently across from the clitoris. Its fibres in a thin layer are spread out and encircle the root of the clitoris, and thence narrowing, proceed out towards the surface of the posterior extremity of the rectus abdominus.

The next in order of position is a longish muscle with a thick round fleshy belly. It arises upon the posterior half of the pelvic bone, and reaching forwards is inserted in front of the vulva. Functionally it is a compressor of the mammary gland, which lies between it and the muscle next to be described. Homologically, from situation and attachments, it is an erector clitoridis or ischio-cavernosus.

Placed more deeply than the last is a much smaller and arciform muscle, also roundish bellied. It is attached to the side of the vulva, behind the mammary gland, and rather lower than the latter. In passing forwards partially under the here imperfectly developed gland, it proceeds to and is fixed upon the deep

pelvic fascia, viz. that which forms a bridge of union between the bones of the opposite sides. This muscle is a true constrictor or sphincter vagina; corresponding to the bulbo-cavernosus of the male.

Lastly, I may take notice of a very large, elongate, thick, fleshy pelvo-caudal muscle; representative of the pubo- and ilio-coccygeus of tailed quadrupeds. This arises partly by tendon and partly by fleshy fibres outside that last described, and from the deep or interior surface of the pelvic fascia. It is continued straight backwards below the erector clitoridis and alongside the rectum. Immediately behind this, having reached the middle line it joins its fellow of the opposite side, and the two form a tapering fleshy mass above a foot long, inserted into the chevron bones, and lying upon the infra-caudal muscles.

I may add that as in globiceps a series of vessels and nerves pass inwards between the pelvic bone and the origin of the ilio-coccygeus. The arteries are derivative of the apparent internal iliac, pudic branches being very distinguishable passing towards the clitoris. The common iliac artery besides, splits into the usual Cetaceæ rete mirabile. Several large glands are situate between and at the anterior ends of the pelvic bones.

In previous whales which I have dissected, I have found some muscular variation from what is here stated, but that and other surrounding structures it is not my intention now to speak of.

3.—*Spiracular Cavity, its Sacs and the Larynx.*

The blow-hole in Risso's Grampus, as in the common Porpoise, is crescentic; concavity forwards. In shape, therefore, the external breathing aperture is unlike that of its supposed ally globiceps, where it is more angular in outline. As in other Delphinæ, where the skull is unsymmetrical, the spout-hole in our specimen was a trifle to the right of the middle line, and opened near, but not quite, at the summit of the head. By measurement in a straight line it was found to be 7 inches behind the tip of the snout, and as much as 12½ inches, following superficially the protuberant curve of the upper lip or maxillary arch.

Communicating with the spiracular cavity, as Von Baer terms the upper common narial passage, are several diverticula

or membranous pouches; the nasal sacs of allied forms. There are three pairs of these, with the addition of a subsidiary sacculate canal on the right. Their wall structures are identical with the same organs in other cetaceans. I have so fully discussed the nature and homology of the nasal sacs in my memoir alluded to, that I will not here enlarge upon the subject. Briefly I may state that I do not regard them as altered and displaced turbinal bones. This view, Von Baer[1] gave forth fully above forty years ago, in a paper full of masterly reasoning, and his notion has not since been contradicted. Indeed Professor Huxley in his Hunterian Lectures[2] favorably inclines to Von Baer's idea. But, as I have shown, they do not tally with osseous elements, but are modifications of membranous sacs met with in the nares of ruminants, and some pachyderms; the two very families used in proof to

Fig. 1.

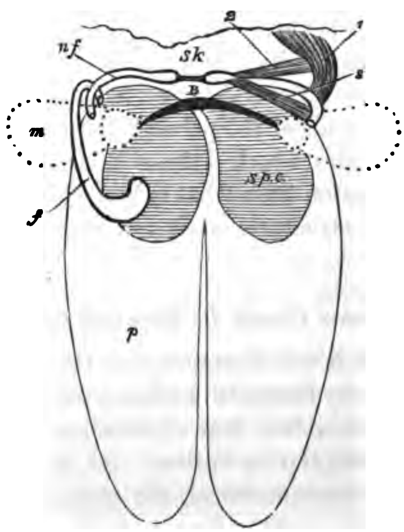


Fig. 1. Diagrammatic representation of the nasal sacs, &c. B. Blowhole: the dark crescent immediately above which the letter is placed. *sp. c.* Spiracular cavity. *p.* Premaxillary sac. *m.* Maxillary sac. *nf.* Naso-frontal sac. *f.* Second or facial division of same. *sk.* Indicates the skull, the waved lined being the coronal suture. 1. 2. 3. The three deep muscular slips acting chiefly upon the naso-frontal sac.

[1] *Der Nase der Cetaceen*, Isis, 1826, p. 811.
[2] Reported in the *Lancet*, 31 March, 1866, p. 350.

demonstrate that the cetacean nasal sacs are homologues of the turbinate bones.

The diagram, fig. 1, illustrates the points of interest connected with the nasal sacs in Risso's Grampus; and I may note that in the terms and lettering I adopt the same used by me concerning analagous parts in globiceps. I have above said of the former, that its sacs are seven in number, and in communication with the spiracular cavity (*sp.*) or common upper nares. Of the membranous chambers the premaxillary (*p.*) (anterior central pouch of Sibson[1] in the Porpoise) is double, long and slipper-shaped. The pair of flask-shaped maxillary bags (*m.*) (anterior lateral pouches, Sib.), as the dotted lines show, are but of moderate size, indeed relatively small compared with what I have met with in fully adult Cete. Each naso-frontal canal (*nf.*) (posterior deep sacs, Sib.) lined with dark pigmentum, has its orifice towards, and opening into, the lateral or maxillary pouch. These so-called naso-frontal cavities are narrow backwardly, and inwardly curved tubes, which mesially terminate in a somewhat bulbous manner, short of each other; but they nevertheless are connected by what appears as a small impervious cord, a central dark puncta being, however, discernible in it. Lastly, I observed, only on the right side though, a secondary tubular prolongation derived from, or close to, the naso-frontal sac. This, for distinction's sake, I denominate the facial division (*f.*) of the naso-frontal sac. In this case it was both longer and larger than the latter; opened by a separate orifice into the maxillary pouch close to, or nearly in conjunction with, that of the post naso-frontal. But unlike the latter it branched forwards, or presented a lateral and crescentiform facial position, ending blindly in a recurved dilatation.

I do not wish to retread the argument used by me, but ere passing from the subject, reiterate conviction of the exceeding likeness of the cetacean naso-frontal sacs to those at the root of the Tapir's proboscis; and the likelihood of the maxillary sacs corresponding to those within the Saiga's[2] nose.

I have expatiated on the homology of the muscles of the blow-hole in globiceps, collating them with those of the epi-

[1] *Philosoph. Trans.* 1848, p. 117.
[2] See *P. Z. S.* 1870, p. 478, fig. 8.

cranial, and naso-facial regions in other mammals. As regards the number of layers, their action, &c., I do not coincide with previous writers. My intention with respect to those of Risso's Grampus, which are in the main similar to its above ally, is merely to add some few remarks on what I have already written and figured, though still unpublished.

In *G. rissoanus* the superficial fleshy sheet, equivalent to an occipito-frontalis, closes the blow-hole by compressing it from before backwards. For, while attached to the occipito-frontal crest behind, it is inserted in front of the sacs converging into a gristly fibrous centre-point.

The second layer, which may be the homologue of the levator labi superioris alaque nasi, has a similar use; and its attachments are the same as in the form compared.

The third layer, functionally acts like the preceding, but compresses more the maxillary sac than influences closure of the blow-hole. This may either be a levator superioris proprius, a levator anguli oris, or the zygomatici—possibly all combined.

The fourth layer, or thin radiating muscular sheet, acts much like a depressor ali nasi, inasmuch as it drags downwards and outwards the parts in front of the blow-hole. It is there fastened by a very broad and thickish aponeurosis, and which, moreover, as it passes outwards overlies the anterior surface of the sac.

Some very short and still deeper fibres in globiceps pass from the naso-frontal sac backwards to the bone behind. In the Grampus, there are three well defined slips in this situation marked 1, 2, 3, in the diagram, fig. 1. The first of these is the broadest, and is directed backwards. The second, a much narrower slip, is attached to the inner end of the naso-frontal sac and passes obliquely outwards and backwards. The third goes more forwards, partly overlying and in front of the cylindrical sac. These three muscles act as compressors and tractors of the sac in virtue of the position they hold to it. They, from situation, if not absolutely function, are homologous with the diminutive bundles which influence the alar cartilages of higher mammals.

The naso labialis or longitudinal semi-fatty muscle, resting upon the upper surface of the premaxilla is very large, and posteriorly it unites with the upper radiate layers in front of the

blow-hole. Use, a protractor and dilator of the blow-hole, as likewise compressor of the premaxillary sac.

Proceeding from the nares to the hyo-laryngeal apparatus—I shall merely note of the basihyal bone, that it has not such regular crescentic proportions as is the case in *Globiocephalus;* but instead is more angular and sharper tipped.

In the Ray Society's translation of that valuable monograph on the Greenland whale, *Balæna mysticetus,* by Professors Eschricht and Reinhardt, the following statement is made (p. 101): "We know that the most essential peculiarity of the whalebone whales, as compared with that of the toothed whales, consists in its allowing the mucous membrane of respiratory canals, by means of an opening on its ventral surface, to appear in the form of a sac with an exterior covering of a strong layer of muscles;" then follow remarks on similar laryngeal sacs in terrestrial mammals.

Now the above seeming distinction between the whalebone and toothed whales, I am prepared to show in the case of Risso's Grampus is one rather of degree than of kind. In short the species under consideration has a laryngeal sinus or small glandular sac opening into the interior of the larynx. In describing this structure I invite attention to the woodcut fig. 2, an exact copy of a sketch made on the fresh object. It represents the interior of the larynx opened, by the wall being cut away a little to the one side of the middle line or towards the observer.

At the lower end of the perpendicular epiglotto-arytenoid passage, the longitudinal folds are here and there interrupted by shallow glandular pits and depressions. Among these, one on the middle of the anterior wall is distinguished from the rest by its forming not a mere crypt, but an orifice leading into a moderate-sized pouch. In the drawing an asterisk marks where a stylet passes through the orifice in question.

The said sac, sinus or deep laryngeal pouch (*Gl. s.*), fills in great part the angle of junction between the enlarged epiglottis and the thyroid cartilage: but it does not reach to the posterior border of the latter. The vacuity or interior hollow of the pouch is of very moderate capacity. In fact, as Eschricht and Rheinhardt say of the enormous laryngeal sac of the Right

Whale, *Balæna*, that while from without it seems very large yet the cavity is comparatively very small; so is it with *Grampus*, where the walls are of considerable thickness. These are composed of a basis of glandular substance partially bound together by fibrous trabecular partitions of smooth mucous membrane, leaving many irregular-sized deepish loculi between.

A sparse padding of fatty substance exteriorly surrounds the pouch, and a thick layer of muscle intervenes betwixt the latter and the inner wall of the cartilaginous thyroid plate. These evidently correspond to those transversely-striped whorled muscular fascicles which surround or form the exterior coat of the so-called air-bag or laryngeal sac, both in the Right[1], the

Fig. 2.

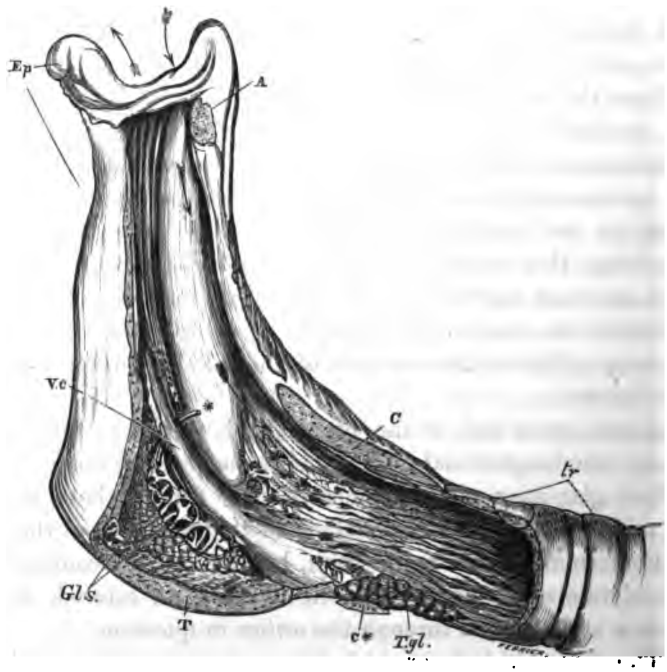


Fig. 2. The larynx in section. The downward arrows denote the posterior more open laryngeal passage; the upward directed arrows guide to the narrower anterior funnel. *Ep.* Epiglottis. *A.* Cut part of the left arytenoid cartilage. *T.* Section of thyroid cartilage. *C. C**. Segments of cricoid. *tr.* Trachea. *T. gl.* Thyroid gland. *Gl. s.* Glandular sac: the stylet (*) shows the orifice of communication with the glottis. *V. c.* Fold or vocal cord.

[1] Vid. Eschricht and Rheinhardt's Engl. Trans. p. 102.

Pike Whale[1] and the Razorback[2]. Their situation and general disposition in Risso's Grampus leads me to suspect they are Thyro-arytenoidei. In the Whalebone Whale they are vastly developed, and follow the disposition of the capacious sac, viz. run backwards betwixt the thyroid and the ventral ring of the cricoid cartilage.

The constituent cartilaginous elements of the larynx resemble much those of globiceps. The tabular prolongation of the epiglottis (*Ep.*) and the arytenoids (*A*) rise to nearly a level with each other; and their adjoining margins are attached quite to the upper flange or lips of their combined orifice. In this they differ from *Balænoptera rostrata*[3], where they are comparatively loose, but agree with *Globiocephalus.*

The body of the thyroid cartilage is a broad flattish plate (seen in section, fig. 2 *T*), and from which alar expansions spread out. These terminate in elongate narrowing inferior cornu attached by ligamentous union laterally and near the posterior border of the cricoid cartilage. The latter cricoid (*C*) presents a ridge and thick arch superiorly, and a narrow backwardly set ring (*C**) inferiorly, which encircles the commencement of the trachea. Here they impinge against, and partly override a small thyroid glandular body (*T. gl.*), which reaches backwards for a short distance beyond.

With the naturalists and anatomists of the first quarter of this century it was a moot question whether the cetacea possessed voice, and in sequence vocal cords. I am not sure indeed if the latter point has been unequivocally settled. That some species utter sound is now unquestioned, and notably the species under consideration and the Ca'ing Whale. As to vocal cords, Meckel[4] strongly opposes Cuvier, and in denial of the latter's statement as well as others, of their being wanting, replies: "Loin de là, l'absence des partes en question se trouve compenseé jusqu'à certain point, à ce qu'il paraît par un autre organisation." With his further description and nature of the parts my own observations coincide; so instead of quotation, I shall refer to fig. 2 of the Grampus's opened larynx.

[1] Carte and Macalister, *Philosoph. Trans.* 1868, p. 238.
[2] Knox, *Cat. Prep. Whale*, p. 11.
[3] Carte, p. 234, and Knox on *Larynx of same species.*
[4] *Anat.* Comp. Vol. x. p. 596.

The tracheal and lower laryngeal mucous membrane folds are numerous and longitudinal. Glandular pits are here and there distributed, and in abundance, opposite the anterior half of the cricoid shield. In the floor of the chamber from about opposite the cricoid, a double raised smooth membranous fold commences, which runs on and upwards to the root of the epiglottis.

As the folds diverge forwards and merge into the narrow rugæ of the upright epiglottic funnel, they are mesially pierced by the orifice of the laryngeal sac already spoken of: around which are glandular crypts. The said parallel folds I take to be the representatives of vocal cords (*Vc*), for reasons I shall presently mention. Above them and on either side of the chamber is a broad, smooth, and long eminence which extends from about the middle of the vocal cords up to near the extremity of the arytenoid cartilage. These eminences and folds thus more or less divide the interior of the erect part of the larynx into an anterior narrow canal and wider posterior channel; the upwardly directed arrows defining the course of the former and the downwardly-placed ones the latter. The abnormally changed relations of the parts, the vocal cords being longitudinal rather than transversely-placed in the Cete, are best studied by comparison with the same region in some ruminants. In *Hyomoschus aquaticus*[1], as Flower has shown, "the vocal cords are placed nearly vertically in the sides of the larynx, and thrown unusually far from its posterior wall, so that when they are brought in contact, a cylindrical tubular air-passage remains open behind them, but the communication between that passage and the thyroid pouch is shut off." I have myself described a similar condition of parts in the larynx of *Saiga tartarica*[2], figuring moreover a longitudinal membrano-fatty eminence, as exists in Risso's Grampus and other whales.

Taking these facts into consideration, and remembering that in the cetacean larynx we have it twisted partially in its long axis, that is a horizontal and vertical portion, and it will be the more readily admitted that vocal cords are not absent. Furthermore, from analogy I am inclined to think that during moderate inspiration the current is chiefly by the posterior channel, but

[1] *P. Z. S.* 1867, p. 957. [2] L. c. fig. 11. B.

by both in a full inspiration. On the other hand, when the cetacean blows with force or utters sound, the posterior division of the chamber is lessened, and exit of air is chiefly through the anterior constricted canal. In watching the movements of the porpoises formerly possessed by the Zoological Society, and as I have witnessed at sea, they occasionally skim the surface; at such times, therefore, gentle inspiration in the manner I have indicated might take place. Immediately after they rise from submergence, however, the blowing is very decided. It would be interesting to note if the voice of cetaceans agrees with such ruminants as approach in form of interior larynx.

The trachea is moderately wide, as are its numerous rings. There is a third bronchus to the right lung, as in other toothed whales. It was with some degree of interest that I examined the lungs to see whether I should discover in them those curious glands at their post-ventral apices figured and described by me at length in globiceps. They exist fully developed, but more than one to each apex, the additional and somewhat smaller bodies lying deeper or behind the others. In shape the lungs themselves comport to those of the Pilot Whale, that is free from incisions and lobes, excepting the tendency to an antero-inferior lobulus. The superficial sinuous vessels are well marked; and the membranous bridge connecting the diaphragm and post-ventral lung-tips of moderate breadth.

4.—*The Alimentary Tract, &c.*

The mouth from the front to its angle is seven inches long. The eaved margin of the maxillary arch overlaps slightly the mandibular one when the mouth is closed. There is a rudimentary roughened callous premaxillary palatal pad. The tongue is smooth-surfaced; its tip distinctly notched, and it is free for 2½ inches from its frænal attachment.

No teeth were visible in this specimen; but in both upper and lower jaw a well-marked dental groove obtained. Laurillard has remarked[1] of the species: "Ces animaux sont sujets à perdre leur dents, et surtout celles de la mâchoire supérieure; aussi n'en connaît-on pas le nombre normal, leur forme est semblable

[1] Fred. Cuvier's *Cétacés*, p. 199.

à celle des dents de dauphin." Risso says[1] the mandible is furnished with five great conical teeth on each side. As this subject is best treated with the osteology, I return to a consideration of the viscera.

The compound stomach of Risso's Grampus corresponds very closely with that of the Pilot Whale, such as I have figured in my memoir of the latter animal. Professor Turner's[2] woodcut, depicting as an anatomical preparation the stomachs of a much younger specimen of the same animal, shows that the relations of the compartments to each other varies from youth to age. That this is the case not only in these aquatic forms, but also obtains in the ruminantia, his researches[3] and the observations of Dr Gedge[4] on the sheep seem to prove.

Comparing the *G. rissoanus* with the specimen of globiceps dissected by myself, I noted in it that the first gastric cavity, as contrasted with the others, was smaller than in the latter cetacean form. This might be as much owing to the animals not having quite arrived at the adult stage as to generic distinction. The second and particularly the third cavity (Turner's 2nd and 4th) were much more elongate or ovoid in *Grampus* than in *Globiocephalus;* the peculiar globular inflation of these cavities in the latter genus being very characteristic. The narrow tunnelled passage betwixt the walls of the second and third gastric cavities (which Turner reckons the third stomach), and connecting these two, has its upper orifice in the Grampus close to that leading from the first to the second cavity.

The long cylindrical fourth cavity and dilated commencement of the duodenum are alike in both forms. This remark applies to the lining of mucous membrane in the whole of the cavities. I may further state, as is a very common occurrence in Cete captured on our coast, there was no food throughout the alimentary canal; mucus, with a yellowish bilious tinge, pervading the upper portion of the gut.

A small compound spleen resembling in shape that of globiceps, lies, as in it, on the right or concave surface of the large first stomach.

The intestines measured 47 feet 2 inches ; thus being seven

[1] *Op. cit.* p. 24.
[2] *Camb. Journ. of Anat.* 1868, p. 73, fig. 2.
[3] *Ibid.* 1869, p. 117.
[4] *Ibid.* 1868, p. 323, Pl. VII. figs. 1—4.

times longer than the entire body: in the globiceps examined by me the proportion was as 9 to 1. Both genera present considerable similitude in the distribution of the intestinal glands, the folds of the mucous membrane, and absence of a cæcum.

A century ago Hunter recognized the considerable uniformity and simplicity of the cetacean liver, and Risso's Grampus offers no exception. In shape, absence of lobulations, smoothness of surface, and no gall-bladder, it agrees quite with the Porpoise and globiceps. Moreover, as in the latter, the bile-duct expands and forms a reservoir within the wall of the duodenum, previous to penetrating the mucous coat of the gut.

The heart offers nothing worthy of remark: there was no accessory orifice and valve in the septal segment of the tricuspid valve, as noticed by me in the Ca'ing Whale. The great vessels spring from the aortic arch as follows: at the summit a short distance apart are a right and left innominata. From the right and behind the deep intercostal is derived. Above this the innominate trifurcates, viz. into a subclavian and double carotid: the same obtains on the left side. The left deep intercostal, however, springs from the left side of the aortic arch at a considerable distance away, and not from the innominata, as on the right side. This arrangement is not unlike what Professor Turner[1] has figured in the Pilot Whale.

In being elongate, loosely acinate, and the vessels entering at the upper end but ventral surface, the kidneys follow *Globiocephalus*.

5.—*Fleshy and other coverings of the body generally.*

The animal being in fair condition, there was as usual a widespread adipose layer overlying the panniculus carnosus. This latter muscle was extensive and of moderate thickness. The facial prominence consisted of material similar to the larger boss of globiceps, but correspondingly less oleaginous in the centre.

Those post-inferior lumbo-caudal masses which by some have been erroneously taken as magnified representations of the iliacus and psoas are large and powerful in *Grampus*. As elsewhere shown, their true homology is with the comparatively

[1] *Loc. cit.* p. 67, fig. 1.

diminutive sacro-coccygeus and infra-caudal muscles of quadrupeds. The dorsal spinal layers offer no departure from other Cete. I may mention, however, trace of a trapezius or rhomboideus capitis, viz. a very thin layer continued on to the occipital crest. The more fleshy rhomboideus proprius passes spinally over the splenius; in some whales a strong aponeurotic fascia alone overlies the latter muscle.

There is a very decided teres major present. A sterno-scapular muscle obtains. This covers the first rib and part of the intercostal space behind; its fleshy belly is more than an inch broad, and the insertion is on the deep surface of the coracoid process. The coraco-brachialis is strong though short.

A few fibres or slender slip representative of a triceps were discernible between the post-humeral border and the infra-glenoidal foot. Nothing which could be construed into remnants of flexors or extensors of the lower fore-limb were observed by me.

The serratus magnus has digitations upon the first four costæ. The external oblique continues forwards to the first rib; but the muscular belly of the rectus abdominis stops at the second rib, and by aponeurosis only proceeds to the anterior rib and sternum. The minutiæ of the fleshy parts of the tongue and hyoidean region I did not enter into, but as they passed through my hands I was convinced of their great resemblance to those of globiceps.

The general results of myological examination of Cetacea lead to the inference that as a whole there is little variation, excepting it may be in minor details. It is asserted, however, that the pike-whale[1] has long flexors and extensors of the fore-arm and manus, an observation requiring confirmation as to its constancy in *Balænoptera* or limitation to the genus.

6.—*Generic affinities as elucidated by the exterior and soft structures.*

In the present phase of Cetology the skeleton has become so much the all-important basis of classification that hints derivable from exterior peculiarities and internal organization are

[1] *Phil. Trans.* 1868, p. 228, figs. 2 and 3, Pl. V.

in a manner overshadowed. Doubtless there is some justification for this in the case of the Cetacea, where a certain sameness of form, a slight gradation of colour, and considerable similarity of interior construction run through the family: besides, essentially accurate delineations of specimens are not overabundant. But is it wisdom to lose sight of the exterior and anatomy, though to skeletal distinctions much of the impetus of modern zoology is due? Let us see in how far externals and internals other than bones tell in the present instance.

G. Cuvier, Risso, and Desmarest[1], have ranged our species under the genus *Delphinus*. Fred. Cuvier[2], with a leaning as above, placed it notwithstanding under his wide group of porpoises and dolphins, *Phocœna*. Lesson and Hamilton[3] refer it to *Globiocephalus*, to which generic division I apprehend Rheinhardt[4], in a passing remark of his, opines, as apparently does Flower[5] in his preliminary note.

Gray[6], though at one time regarding it as mayhap his *G. svineval*, has since generically put it under *Grampus*, and most reasonably says of Lesson's choice, "but the position of the dorsal and the form of the pectoral as well as the description of the teeth make me believe it rather belongs to this genus (Grampus)." Van Beneden and Gervais[7] follow Gray, but Gervais himself in his earlier memoirs[8] classed it as a *Delphinus*.

My own first impression on seeing the body was the idea of a magnified porpoise, with a dash of the deductor and dolphin. I then compared it with photographs and most carefully executed coloured drawings of the porpoise and globiceps in my possession.

In analysis of the features, the colour aside from specific scratch-markings, decidedly is akin to *Phocœna*. By lightening the shade of the latter, giving it a paler forehead and darkening the subcaudal region, the general effect would agree. Again, this grampus shows leanings to the dolphins, where abdominal whiteness, irregular slashing of same, and iridescent hues are

[1] *Encycl. Mam.* p. 519. [2] *Hist. Nat. des Cétacés*, p. 208.
[3] In Jardine's *Nat. Lib.* 'Whales', p. 219, Pl. XVIII.
[4] 'On Pseudorca crassidens,' Ray Soc. Transl. 1866, foot-note, p. 216.
[5] *P. Z. S.*, 1870, p. 128.
[6] *Erebus and Terror*, also *Cat. B. M.* 1850 and 1866.
[7] *Ostéographie des Cétacés.* [8] *Zool. et Paléont.* &c.

far from uncommon. The orcas come between them and globiceps, inclining to deepening of tint; in the latter a sooty black.

The shape and position of the fins are relevant to generic conception, inasmuch as in *Delphinus* the dorsal is falcate and placed about the centre of the body. *Pseudorca*[1] and *Orca* nearly agree, and are intermediate between it and *Grampus;* where, also high, it is slightly behind the middle. In *Globiocephalus* it is lower, broader, and a trifle in front of the middle point. *Phocæna,* while having it if anything behind the middle, has it relatively much broader and wanting the deep scooping of the posterior border.

As is well known, *Globiocephalus* has a long, narrow, acute pectoral fin, the extreme of the short, relatively broad one of *Phocæna. Delphinus,* as far as I can judge from the accounts of others, would take an intermediate place, but nearer to the latter. Regarding the general "cut," as Rheinhardt deftly expresses it, *Pseudorca's* resembles the Ca'ing whale, but its breadth is greater in proportion to its length. In these respects *Grampus rissoanus* rather agrees than differs from *Pseudorca. Orca* has a broader fore-limb than either. The *D. griseus,* Cuv. (*Grampus cuvieri* of Gray), though doubtless the nearest ally to Risso's species, has oddly enough by D'Orbigny's computation an extraordinary long pectoral, which G. Cuvier's words in part contradict, as certainly does his figure. In fact the words "moins qu'au globiceps[2]" point to similarity with *G. rissoanus*[3].

Cuvier recognised globiceps by its enlarged, protuberant blunt muzzle. In the grampus this is much less truncate and nodose, in our specimen comparable to the outline of a parrot's beak. In the killers and the porpoise it becomes very sensibly diminished and backwardly inclined, and in the dolphins runs into an elongate flattened beak, particularly in such forms as *Tursio* and *Lagenorhynchus.*

There is but a single premaxillary nasal sac in *Phocæna;* in *Grampus rissoanus* as in *Globiocephalus* it is double.

[1] Consult Prof. Rheinhardt's very appropriate Remarks on *P. Crassidens*, Ray Soc. p. 217. His stricture on Gray's generic character of fin, however, I do not agree with.

[2] See Rheinhardt's criticism on this very subject, *l.c.* p. 216 and 217, foot-note.

[3] Which Pl. 54 as referred to in the *Ostéographie* well brings out.

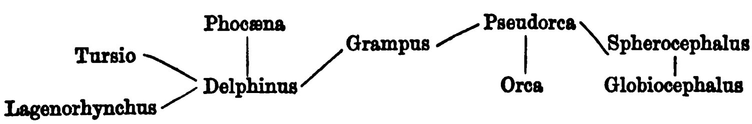
The patterns of the larynx and hyoidean apparatus come nearest in the two latter. Each of them has those peculiar enlarged glands at the ventral diaphragmatic ends of the lungs; and these are certainly wanting in *Phocæna* and *Delphinus* (*Delphis ?*), specimens of which were compared along with *Globiocephalus* by Jackson[1], who distinctly mentions their presence in the latter.

The number of compartments of the stomach and their relation to one another is typical of both the two last-mentioned genera; and is liker them than the one preceding. The same family connection is shown in the kidneys, where the blood-vessels enter at the fore and inner or atlanto-mesial end.

The teeth, few and early deciduous, are an important feature; but as our specimen was edentulous or at least non-erupted, I leave out their consideration as a point to base judgment on.

To sum up, as far as my examination extends, the specimen in question has its nearest alliance with and indeed would appear to be a species of the genus *Grampus* as defined by Gray; which group, however, while clear to me, it may be remarked is far from being isolated or rigidly circumscribed. On the one hand it has many solid points of connection with *Pseudorca* and *Orca;* other characters draw it towards *Globiocephalus.* It has again more than mere resemblances to the genera of true dolphins and to the porpoise, though possibly further removed from them than the killers.

When Professors Van Beneden, Gervais and Flower have worked out its entire osteology, the data will enable a sound comprehension of its affinities to be formed; provisionally therefore I offer the subjoined scheme of relationships.



P. S. In quoting Professor Turner's paper for 1869, I did so in my MS. from a knowledge of its contents, not having the Journal by me at the time; but in correcting the proof neces-

[1] *Bost. Journ. Nat. Hist.* Vol. v. p. 137.

sarily made it a point to certify the statement and number of page. In doing this my eye caught a reference of his to "*Note sur un Cétacé Grampus griseus*" par. M. P. Fisher, in *Ann. des Scien.* Nat. 5th Ser. 1867, p. 363. I therefore read this, and I confess with much interest. Fisher's description of the animal examined by him leads me to believe it was identical with that dissected by myself, inasmuch as colouring, streaks or scratch-marks, and form of head and body are concerned. His remarks on the stomach as well as Turner's confirm what I have above said of certain genera individually examined by me. Fisher tersely compares the skeleton and dentition of his supposed Cuvier's with Risso's species, and speaks of their geographical range and migrations. He sums up with four conclusions and a corollary. Without giving his own words I may curtly express his ideas as follows: 1. *G. rissoanus* appears in the Mediterranean about the same period of the year that *G. griseus* is found on the French Atlantic coast. 2. Their dentition is nearly alike the latter with 2 to 4 mandibular teeth on each side, the former 5 to 6. 3. Their bones are identical. 4. Difference of colour accounted for by its being variable, and shape by post-mortem effect. Hence he is of opinion the two species ought to be re-united, as Baron Cuvier was inclined to regard them.

It appears to me that M. Fisher from his stand-point has not overdrawn legitimate inferences, provided that it was *G. griseus* and not *G. rissoanus* he examined. As to variation of colour in what has been designated under the latter title, what I have mentioned proves, and Van Beneden and Gervais' plate shews great likeness in osteological features; so that I am prepared to find, when the latter has been carefully compared in the young and old, that they may after all be but one specific form, though Dr Gray lays stress on the black colour of his *G. cuvieri* = *D. griseus* Cuv.

ON THE SPECIFIC HEAT OF BLOOD. BY ARTHUR GAMGEE, M.D., F.R.S.E., *Lecturer on Physiology at Surgeon's Hall, Edinburgh, and Physician to the Royal Edinburgh Hospital for Sick Children.*

(Extract from a Report presented to the Physiological Sub-Section of the British Association. Liverpool, 1870.)

BEFORE commencing independent experimental researches with a view to determine the heat of arterialization, it appeared to me to be essential to undertake a set of experiments with the object of determining with accuracy the specific heat of blood; and it is to a notice of these that I at present confine myself, reserving the account of experiments, now in progress, on the further question of the heat of arterialization to a future occasion.

I believe I am quite accurate in saying, that the specific heat of blood has been determined by Dr John Davy alone, his experiments being recorded in his *Observations Anatomical and Physiological,* in a chapter entitled, "On the Capacities of venous and arterialized Blood for heat." In his experiments he made use of defibrinated blood, and employed for the determination of specific heats the methods of mixture and cooling. According to Davy, the specific heat of lambs' and sheeps' blood varied from 0·812 to 0·934 (water being 1·00).

The great discrepancy of these results made it most desirable that the experiments should be repeated in the most careful manner.

I employed invariably the method of mixture:

A flask furnished with a tubulature near its base was fixed in the centre of a water-bath, and from the flask a tube also surrounded by water opened externally by a stop-cock. This flask was filled with mercury.

The blood to be experimented upon was placed in a light and highly polished iron vessel weighing 92·15 grammes, which was surrounded by cotton-wool and placed in a glass beaker. The temperature of the blood and mercury was ascertained

before and after mixture by means of a standard thermometer made by Fastré of Paris, belonging to the Museum of Natural Philosophy of the University of Edinburgh, divided into fifths of a degree centigrade, and capable of being read to one-fiftieth of a degree.

In my experiments warm mercury was added to blood at a lower temperature. The specific heat was determined by the usual formula:

c = coefficient of the specific heat of blood.
M = weight of blood.
T = its temperature.
m = weight of mercury.
t = its temperature.
c' = coefficient of specific heat of mercury, *i.e.* 0·033.
θ = temperature of mixture of blood and mercury.
μ = specific heat of iron vessel.

$$C = \frac{m\,(t-\theta)\,c'}{(M+\mu)\,(\theta-\tau)}.$$

The results of my experiments are exhibited in a tabular form below:

No. of experiments.	Blood.	M.	T.	m.	t.	μ.	θ.	c. sp. heat.
1	Ox.	76·15	12°·00	grammes 1756·7	36°·4	10·48	21·8	1·00
2	do.	249·95	18°·45	1778·9	39°·8	10·48	22°·15	1·07
3	do.	224·35	16°·30	1671·05	41°·2	10·48	20°·8	0·99
4	do.	224·05	22°·0	1980·5	46°·8	10·48	27°·15	1·06*
5	do.	283·60	23°·0	2303·2	47°·0	10·48	27°·80	1·03
6	do.	211·55	15°·5	1439°·2	49°·1	10·48	21°·70	0·97

* This blood had a specific gravity 1058 at 55·7 Fah. Solids per 1000, 213·10.

As a check to the accuracy of the method and apparatus employed in the above experiments, I determined the specific heat of mercury by means of it, substituting water for the blood used in my previous experiments. Only two experiments were performed.

1. $M = 252{\cdot}55$ grms. of H_2O.
   $T = 10^\circ{\cdot}5$.
   $m = 1413{\cdot}8$ grms. of Hg.
   $t = 49^\circ{\cdot}0\,C$.
   $\theta = 16^\circ{\cdot}0\,C$.
   $\mu = 10{\cdot}48$ $C = 1$ $c' =$ coefficient of sp. heat of mercury.

   $$c' = \frac{(M+\mu)\,(\theta - T)\,c}{m\,(t-\theta)}.$$
   $$= 0{\cdot}0322.$$

2. $M = 286{\cdot}75$.
   $T = 12^\circ{\cdot}3$.
   $m = 1226{\cdot}6$.
   $t = 37{\cdot}0\,C$.
   $\theta = 15^\circ{\cdot}15\,C$.
   $\mu = 10^\circ{\cdot}48$.
   $c' = 0{\cdot}0315$.

The above determinations were all made with the perfectly fresh blood of the ox. They may, I think, be taken as representing very accurately the specific heat coefficient of the blood. They shew that the specific heat of blood is not, as Davy supposed, considerably below that of water, but almost exactly the same, the mean of all my results giving the coefficient of the specific heat of ox-blood as 1·02.

A knowledge of this was essential to the further progress of the research on the heat developed during the arterialization of the blood, with which I am at present engaged.

RESEARCHES ON THE CONSTITUTION AND PHYSIOLOGICAL RELATIONS OF CYSTINE ($C_3H_6NO_2S$).

By James Dewar, F.R.S.E., *Lecturer on Chemistry, Veterinary College, Edinburgh;* and Arthur Gamgee, M.D., F.R.S.E., *Lecturer on Physiology at Surgeon's Hall, Edinburgh.*

Amongst the rarest and least understood of the proximate principles of the animal body, must undoubtedly be placed the substance which forms the subject of the present paper.

Cystine is a body which is best known as a constituent of urinary calculi, and of the urine, and which has been stated to exist in the substance of the kidneys. When present in the urine, cystine generally deposits after some time in the form of individual hexagonal plates; sometimes these are grouped together in large masses and exhibit a great tendency to adhere to the sides and bottom of the vessel in which the urine is contained. These crystals are readily soluble in solution of ammonia, which may be used as a convenient re-agent for separating and purifying the substance.

Although occurring in the urine, there can be no doubt that its presence is very rare, and when it does occur it is in the urine of persons who are the subjects of the so-called cystine diathesis, *i.e.* in whose urine it has a great tendency to make its appearance.

Sometimes the individuals who are the subjects of the so-called cystic diathesis are apparently less robust than they should be, whilst in other cases nothing is observed to indicate any disturbance in the functions of the body, until urinary symptoms point to the existence of a calculus.

The sweat undoubtedly contains cystine in some cases. One of us had the opportunity of detecting cystine in the perspiration of a man under the care of Professor Maclagan in the clinical wards of the Royal Infirmary, and we are in a position to state positively that in cases where cystine is present in the urine, silver coins carried in the pockets of the patients have been observed rapidly to become blackened.

Since the time when Wollaston (*Philosophical Transactions*, 1810) discovered cystine, we have been made acquainted with its elementary composition, with its physical properties, and with its deportment when treated with several re-agents, still we have not as yet been placed in possession either of clinical observations or of chemical facts which serve to connect cystine with any particular chemical operation going on in the living body. From the ultimate analyses which have been made by Liebig and others, the formulæ $C_3H_6NO_2S$ and $C_3H_7NO_2S$ have been ascribed to it by different authors. On comparing the results of standard analyses we are, however, convinced that the former and older is the correct formula, and it is the one which is supported by the experiments which we have made.

Although it may at first sight appear that the research upon which we are engaged is only of interest from a purely chemical point of view, a more careful consideration will shew that a knowledge of the chemical constitution is the first essential to our forming any hypothesis as to the relations of cystine to the animal processes, and that indeed our experiments already point to the probable seats of the formation of cystine. We shall allude to the subject in our concluding remarks.

The cystine used in our experiments was obtained from two portions of calculi, one of which was furnished to us by Professor Maclagan, the other by the Royal College of Surgeons of Edinburgh. The cystine was separated by treating the pounded calculi with strong liquor ammoniæ, which dissolved the greater part, and allowing the solution to evaporate at a very gentle heat. The cystine which separated was then again dissolved in ammonia and recrystallised.

Preparation of Hydrochlorate of Cystine.

One gramme of cystine was dissolved in boiling hydrochloric acid; on cooling beautiful needle-shaped crystals separated, which were very soluble in water. When thoroughly dried in vacuo over quicklime the crystals were found not to be readily soluble in water. 0·05 grm. of crystalline hydrochlorate of cystine yielded 0·0452 grm. of AgCl, corresponding to 22·2 per cent. of HCl (Calcd. 22·5).

Action of Nitrate of Silver on Cystine.

Cystine was dissolved in strong solution of ammonia, and to the solution was added a solution of silver nitrate in ammonia. No precipitate occurred, nor did the solution darken in the cold. When slightly acidified with nitric acid, a canary-yellow precipitate was thrown down, which was collected and dried *in vacuo.* The filtrate blackened when heated, and on filtering off the black precipitate a clear colourless solution was obtained, which was not further blackened when boiled with ammoniacal solution of oxide of silver.

On analysis the substance precipitated proved to be a compound of cystine with nitrate of silver.

In a subsequent experiment an ammoniacal solution of cystine was boiled with an ammoniacal solution of nitrate of silver. A black precipitate fell which consisted of sulphide of silver. The filtrate from the precipitate of sulphide of silver was subsequently treated with solution of chloride of ammonium to separate the excess of silver. The solution was found not to be precipitated by hydrochloric acid and chloride of barium, nor by sulphate of calcium. It is therefore evident that when an ammoniacal solution of cystine is heated with ammoniacal solution of oxide of silver, the sulphur is separated entirely as sulphide of silver, none being oxidised; it is also obvious that no oxalic acid is formed.

Action of Caustic Soda and Caustic Baryta on Cystine.

Cystine, when treated with pure solution of pure NaHO, and evaporated in a silver basin, gives a reddish fluid; sulphide of sodium is then produced, blackening the basin, and ammonia is copiously evolved. On treating the residue with water, neither sulphuric nor oxalic acids can be detected. The liquid contains, however, a large quantity of sulphide of sodium with a mere trace of sulphite.

Cystine, when heated to 150° C. with solution of caustic baryta in sealed tubes, gave off ammonia, a large quantity of sulphide of barium, a smaller quantity of sulphite of barium, and a trace of hyposulphite being formed. No trace of sulphocyanide could be detected.

Action of Alcoholic Solution of Potash on Cystine.

Cystine was heated for several hours in a sealed tube at 130° C. with an alcoholic solution of potash. At the conclusion of the experiment a small quantity of dark sticky matter was found adhering to the tube, which contained a yellowish fluid. The latter smelt strongly of ammonia, which was separated by distillation. The residue was acidified with dilute sulphuric acid, and shaken up with ether. Ether left a yellow non-crystalline substance, possessed of an indefinite but disagreeable odour. This substance had a strong acid reaction, and was found to contain no sulphur.

Action of Nascent Hydrogen on Cystine.

When cystine is added to a mixture of tin or zinc and dilute hydrochloric acid, large quantities of sulphuretted hydrogen are given off; the evolution of gas gradually slackens, till even after the action has gone on for several days, traces of sulphuretted hydrogen continue to be given off. When treated in the same manner taurine does not evolve H_2S.

It is to be noted that this evolution of H_2S, when cystine is treated with tin or zinc and hydrochloric acid, might be used as a test for the substance, care being previously taken to separate any sulphide which might exist.

Action of Nitrous Acid on Cystine.

Some of the most important facts with regard to the chemical reactions of cystine have been recorded by Dr Bence Jones, who for the first time showed that nitrous acid decomposes it with the evolution of nitrogen, and that in this operation the sulphur which it contained is oxidised to sulphuric acid, whilst a non-crystalline substance is left which is precipitable by nitrate of silver, mercuric chloride, as well as by acetate of lead.

In our experiments we sometimes acted upon solution of pure hydrochlorate of cystine by means of nitrite of silver; in other experiments we treated pure cystine with a stream of nitrous acid gas.

Cystine was placed in water and a stream of nitrous acid gas passed through it. No action took place until the water was heated; it then commenced and proceeded briskly, with abundant effervescence, until the whole of the substance was dissolved.

The clear solution contained a large quantity of sulphuric acid, but not a trace of oxalic acid. When boiled with an ammoniacal solution of nitrate of silver, considerable reduction took place, a beautiful mirror of silver being deposited on the glass. The fluid was again subjected to the action of nitrous acid; still no oxalic acid could be found, and the reduction of an ammoniacal solution of oxide of silver continued. A portion of the fluid was treated with carbonate of barium and heated; the clear filtrate had an alkaline reaction, and was abundantly precipitated by nitrate of silver and acetate of lead. The remainder of the fluid, after the treatment with $BaCO_3$, was treated with solution of nitrate of silver. An abundant canary-yellow precipitate was formed. This was suspended in water and decomposed with H_2S; the filtrate was evaporated to dryness, and presented the appearance of a sticky solid. It was soluble in water. The aqueous solution was evaporated and treated with absolute ether, which dissolved the greater part. The ethereal solution left on evaporation an acid fluid. This was dissolved in water, neutralised with ammonia, and precipitated with solution of nitrate of silver. The yellow precipitate obtained was amorphous; it was dried in *vacuo.* Two specimens of the silver salt prepared at different times were analysed by us. The following are the results of two analyses :—

Silver . . .	56·9	57·5
Carbon . . .	19·43	21·32
Hydrogen . .	5·29	4·64

In considering the discrepancies of these analyses, it must be borne in mind that we were operating on excessively small quantities of a substance prepared at different times by complicated processes.

Remarks.

Cramer believed that cystine was intimately related to the body called serin, $C_3H_7NO_3$, which is obtained as one of the

products of the action of alkalies on silk. Serin, when treated with nitrous acid, yields glyceric acid, as alanine under the same circumstances yields lactic acid, and therefore serin may be looked upon as amido-glyceric acid.

Cramer further believed that cystine was a sulpho-amido-glyceric acid, *i.e.* serin in which hydroxyl has been replaced by HS.

This supposed relation is exhibited below :—

CH_2OH	CH_2NH_2	CH_2NH_2
$CHOH$	$CHOH$	$CHSH$
CO_2H	CO_2H	CO_2H
Glyceric Acid.	Amido-glyceric Acid or Serin.	Cystine.

Considering that this relation of cystine to serin really exists, some have argued that on treatment with nitrous acid, cystine should yield glyceric acid. We do not, however, admit that this would really be the case. If we examine the case of sulpho-lactic acid, an analogous body to the supposed sulphur derivative of serin, we find that, on oxidation, it gives sulpho-propionic acid, and therefore we should in the case of cystine expect that a sulpho-acid would be formed on treatment with nitrous acid, were it built up as Cramer supposed. We have uniformly observed, during the course of our experiments, that, however carefully we attempted to regulate the action of nitrous acid on cystine, or of a nitrite on a salt of cystine, the sulphur separated as sulphuric acid, thus pointing to a material difference in its reactions from what we should have expected from its supposed constitution.

In a paper brought before the Royal Society of Edinburgh (May, 1870) we stated that although we could not consider our experiments to be definitive, we were in a position to state that glyceric acid was certainly not a product of the action of nitrous acid on cystine, and we ventured to predict that cystine would be found to be related to pyruvic acid, and to be an amido-sulpho-pyruvic acid.

We based our supposition on the near approach of the analyses of the silver salt of the acid obtained by the action of nitrous acid on cystine to the composition of pyruvate of silver,

and to the general characters of the oily acid produced. Since then we have compared the reactions of pyruvic acid prepared by us with those of the acid obtained from cystine, and we have no doubt of their identity.

Pyruvic acid was discovered by Berzelius as one of the products of the dry distillation of racemic and tartaric acids, and has been called pyroracemic acid. To prepare it, tartaric acid is distilled at a temperature gradually rising to 300° C., and the oily acid distillate thus obtained is repeatedly fractionated. The portion distilling between 160° C. and 170° C. is found to have the composition $C_3H_4O_3$.

Pyruvic acid is a liquid of a yellowish colour smelling of acetic acid, slightly viscid, boiling at 165° C., always with some decomposition. It is soluble in water, alcohol and ether.

This acid differs from lactic acid in containing a molecule less of hydrogen, and has been shewn by Wislecenus and Debris to be easily transformed into lactic acid by hydrogening agents, such as nascent hydrogen and hydriodic acid. It also unites directly with a molecule of bromine.

The following are the molecular formulæ of ordinary lactic acid, and pyruvic acid: to the right is exhibited a probable molecular formula of cystine:

Lactic.	Pyruvic.	Cystine.
CH_3	CH_3	CH_2 (H_2N)
$CHOH$	CO	CS
CO_2H	CO_2H	CO_2H

It is obvious that the above molecular formula of cystine exhibits only one of at least four possible isomers.

One of the peculiarities of pyruvic acid is the ease with which it is transformed by heat into a non-volatile syrupy acid which is undoubtedly a polymeric modification. This new acid is also produced when pyruvic acid is separated from its salts; salts are not crystallizable, and they are of a peculiar yellow colour.

A greal deal more might be said touching the theoretical relations of pyruvic acid to acetone and its derivatives; but this would be quite out of place here.

The relation which we believe we have shewn to exist

between cystine and pyruvic acid, and consequently between cystine and lactic acid, is not without interest to the physiologist. The juice of muscle has long been known to be rich in an isomer of lactic acid, viz. sarco-lactic or paralactic acid, and we now know that this acid is one of the products which are formed in muscle during muscular work. The living and inactive muscle has an alkaline reaction, but when it is made to contract it gradually becomes acid, and evolves carbonic acid; the sarco-lactic and carbonic acids being developed by the splitting up of a substance whose presence in muscle is apparently essential to the production of mechanical work. When a muscle passes into the condition of rigor mortis the same changes occur, and we know that at the same time heat is evolved.

Sarco-lactic acid is, as we have said, merely an isomer of ordinary lactic acid, and readily passes into it. We cannot but think that in all probability cystine is a product of the splitting up of an albuminous constituent of muscle—that it represents an imperfect oxidation in muscle, by which a portion of the sulphur residue of an albuminous body leaves the muscular fibre in an unoxidized condition.

NOTES OF SOME EXPERIMENTS ON THE RATE OF FLOW OF BLOOD AND SOME OTHER LIQUIDS THROUGH TUBES OF NARROW DIAMETER. By J. MATTHEWS DUNCAN, M.D., F.R.S.E. and ARTHUR GAMGEE, M.D., F.R.S.E.[1]

THE experiments, of which the results are recorded in the present communication, were undertaken in order to determine the rate at which blood flows through tubes of moderately small diameter, with a view to the study of the mechanical theory of dysmenorrhæa; they were afterwards extended to blood-clot serum, milk, and urine, &c.

In a memoir inserted in the ninth volume of the *Mémoires des savants Etrangers*, M. Poiseuille stated the results of an investigation on the flow of water and other fluids through capillary tubes, showing how this is influenced by pressure, by the length and diameter of the tube, and by temperature. A committee of the French Academy, of which M. Regnault was the reporter, corroborated the results of M. Poiseuille's researches[2]. Subsequently this observer published a still more extended series of observations, including the determination of the rate of flow of serum and defibrinated blood[3].

The method employed by Poiseuille in his researches, and which is described at length in his Memoir, consisted essentially in causing air under a known pressure to force a known quantity of the fluid to be experimented upon through tubes of known diameter and length, and determining the time employed.

The following are the general results at which he arrived concerning the influence of the length and diameter of tubes of smaller diameter than a millimetre on the rate of flow of any liquid at a constant pressure and temperature:—

1st. The volumes of liquid flowing in equal times through capillary tubes of equal length, but of different diameters, are amongst themselves as the fourth powers of the diameters.

[1] Communicated to the Royal Society of Edinburgh, May, 1870.

[2] Recherches expérimentales sur le mouvement des liquides dans les tubes de très-petits diamètres. Commissaires MM. Arago, Babinet, Piobert, Regnault rapporteur. Académie des Sciences, séance du 26th Décembre 1842.

[3] Recherches expérimentales sur le mouvement des liquides de natures différentes dans les tubes de très petits diamètres par M. le Dr Poiseuille. *Annales de Chimie et de Physique*. Troisième série, t. xxi. 1847.

2nd. The volumes of liquids which flow in equal times through capillary tubes of the same diameter, but of different lengths, vary inversely as the length of the tubes.

With regard to the influence of pressure, it was found that the rate of flow increased directly as the pressure; and with regard to the temperature, that, *as a general rule*, the rate of flow of solutions increases as the temperature rises.

With regard to the influence of various substances held in solution by a fluid, on the rate of flow no general law was arrived at, connecting it either with chemical constitution, density, capillarity, or viscosity[1].

The following are some of the results, extracted from M. Poiseuille's Memoir:

Tube employed (B) is 64 millimetres long; its diameter is $0^{mm} \cdot 249$; capacity of receiver, 6 C. C.; pressure, 1 metre; temperature, 14°5 C.

	Time of flow. s.
1. Distilled water	535·2
2. Ether	169·0
3. Alcohol	1184·5
4. Serum of ox's blood . . .	1029·0

M. Poiseuille made a single determination of the rate of flow of blood-serum; of blood-serum plus a small and unknown quantity of corpuscles, and of defibrinated blood, the same animal's blood (an ox's) having been used to furnish the three liquids. The following are the results:

Temperature and pressure stated to have been kept constant during all the experiments; length of tube, 110 millimetres; diameter, $0^{mm} \cdot 256$; capacity of receiver, between 5 and 6 cc.

	Time of flow. m. s.
Serum	20·33
Serum containing a small and unknown quantity of blood-corpuscles . .	21·17
Defibrinated blood . . .	68·47

[1] We may merely allude to the fact that M. Graham succeeded in showing a decided connection between the rate of flow of the different hydrates of sulphuric acid and their chemical constitution. His very interesting results are to be found in a paper "On liquid transpiration in relation to chemical composition." (*Philosophical Transactions*, 1861, p. 373.)

Poiseuille points out that the aggregation of blood-corpuscles, which always takes place in defibrinated blood, leads to a choking of the tubes employed, especially when these are of narrow diameter ($0^{mm}\cdot 1$), or to an irregular flow, and that consequently defibrinated blood cannot readily be injected through the capillaries of the lungs of animals which have been bled to death. The recent experiments of Dr J. J. Müller[1], carried on under the direction, and according to the method, of Professor Ludwig, in the Physiological Institute of Leipzig, are opposed to the statement of Poiseuille, for he succeeded in keeping up for long periods a flow of defibrinated blood through the lungs.

Method employed in the present research.

All our experiments were conducted according to a method suggested by, and under the direction of, Professor Tait, in the Physical Laboratory of the University of Edinburgh. The liquids to be experimented upon were allowed to flow through tubes of known diameter and length, into a large air-pump receiver exhausted to a partial and known extent, the fluid being thus subjected to the pressure of the atmosphere, minus that of the air in the receiver.

Before enumerating our experiments, it may be well to point out certain fundamental differences which exist between them and those of M. Poiseuille. 1st, our tubes had a much wider diameter—those used by the French observer varied in diameter from $0^{mm}\cdot 1949$–$0^{mm}\cdot 256$, whilst our tubes were from $0^{mm}\cdot 845$–$1^{mm}\cdot 259$; 2dly, by our tubes being much longer than those of Poiseuille; and, 3dly, by the liquids being allowed to flow, not into water, but into empty vessels placed in the partially exhausted receiver.

I.—*Influence of the Shape of the Tubes employed on the Rate of Flow.*

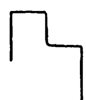
It was considered advisable to determine, in the first place, whether bends in the tubes through which the liquids were made to flow would exert any influence on the rate. Accordingly, a tube 1129 millemetres long was bent twice at right

[1] "Ueber die Athmung in der Lunge von Dr J. J. Müller." Arbeiten aus der Physiolog. Anst. zu Leipzig Mitgetheilt durch C. Ludwig. Leipzig, 1870, pp. 37—76.

angles; one end was connected by means of a tightly fitting cork with the exhausted receiver, and the other was at a given instant immersed in water. The rate of flow having been determined, the tube was bent four times at right angles, and the experiment repeated; then it was not only bent four times at right angles *in one plane,* but bent at one point at an angle of about 135° to its former plane.

The results of these various experiments are exhibited in Table I.

TABLE I.

No. of Experiments.	Fluid used.	Diameter of Tube.	Length of Tube.	Temperature.	Pressure.	Time of Flow of 100 Cubic Cents. in Seconds.	
		mm.	mm.		mm.		
1–5	Water . .	0·845	1129·8	13°·0 C	708·59	126·4	Tube bent twice at right angles, thus, 
5–8	Common Sulphuric Acid	"	"	13°·5	"	2978·0	
8–9	Water . .	"	"	13°·5	588·5	158·0	
10–11	Water . .	0·845	1129·8	13°·5	588·5	159·0	Tube bent four times at right angles in the same plane, thus, 
11–12	Water . .	0·845	1129·8	11°·5	588·5	157·4	Tube bent four times at right angles; at one point bent at an angle of about 135° to its former plane.
13–14	Water . .	0·845	1129·8	11°·4	588·5	161	Tube again bent as in experiments 10 and 11.
15–17	Water . .	0·845	1129·8	33°·0 C	588·5	108	

It results from these experiments that the bends in the tubes had no perceptible influence in modifying the flow—the quantity of fluid flowing in the same time being directly as the pressure, and very much influenced by rises of temperature.

II.—*Rate of Flow of Defibrinated Blood of Sheep.*

Having determined that the shape of the tubes exerted no influence on the flow of fluids through them, we proceeded to examine the comparative rate of flow of the defibrinated blood of the sheep. The results are recorded in Table II.

The tube used in this experiment was 908·9 millimetres long, and was twice bent at right angles. The diameter was 1·214 millimetres.

TABLE II.

No. of Experiments.	Fluid used.	Diameter of Tube.	Length of Tube.	Temperature.	Pressure.	Time of Flow of 100 Cubic Cents. in Seconds.
		mm.	mm.		mm.	
18–21	Water . . .	1·214	908·9	10°·5	583·5	67·6
22–25	Defibrinated sheep's blood . . .	”	”	16°·7	583·5	227·6
26–28	”	”	”	”	”	
29–31	”	”	”	”	”	
32–35	”	”	”	31°·0	”	143·4

TABLE III.

Comparative Rate of Flow of Water, Defibrinated Ox-Blood, Serum of Blood (obtained from same sample of Blood), and Defibrinated Sheep's Blood.

No. of Experiments.	Fluid used.	Diameter of Tube.	Length of Tube.	Temperature.	Pressure.	Time occupied by Flow of 100 Cubic Cents. in Seconds.
		mm.	mm.		mm.	
36	Water . . .	1·214	908·9	12°·0 C	598·7	68·16
37[1]	Serum of ox-blood	”	”	13°·1	”	97·10
38	”	”	”	”	”	98·14
38	”	”	”	16°·0	”	94·50
40	Defibrinated ox-blood . .	”	”	”	”	365·7
41[2]	Defibrinated sheep's blood	”	”	18°·0	”	260·2

[1] Solids in 1000 parts of serum . . . . 90·41
Water . . . . 909·59
[2] Solids in 1000 parts of the blood . . . . . . 212·21
Water . . . . 787·79

III. *On the Rate of Flow of Pure* (i.e. *uncoagulated*) *Blood at the Temperature of the Body through Narrow Tubes.*

Exp. 43.—In this experiment a calf, about a week old, was made use of. The jugular vein on the left side having been exposed, an opening was made into it as low in the neck as possible, and a flexible catheter was passed into the right side of the heart; the venous blood used was thus obtained.

Thereafter the carotid artery was exposed on the same side, and a ligature having been applied on the distal side of the exposed portion, a tube was introduced into the cardiac end. From this tube was obtained the arterial blood used in the experiment.

The temperature of the calf before the experiment was, 38°·8 C.
After the experiment . . . . 38°·7 C.

The blood was received directly into graduated tubes heated to 38°·8 C.

Two narrow tubes were used in these experiments. Their length was 56 inches. The first (Tube C) had a diameter of 1·259 of a millimetre. The second (Tube A) had a diameter of 0·9289 of a millimetre.

TABLE IV.

No. of Experiments.	Fluid used.	Diameter of Tube.	Length of Tube.	Temperature.	Pressure.	Time of Flow of 100 Cubic Cents. in Seconds.
		Tube C. mm.	mm.		mm.	
43	Water . . .	1·259	914·	15°· C	601·7	42·10
44	Water . . .	,,	,,	39°·5 C	,,	39·43
45, 46	Venous blood of calf	,,	,,	38°·8	589·0	54·9
46, 47	Venous blood of calf, defibrinated and arterial .	,,	,,	,,	,,	53·11
48, 49	Arterial blood of calf	,,	,,	,,	,,	60·07
		Tube A.				
50	Water . . .	0·9289	914·	38°·5	601·7	69·4
51–53	Arterial blood of calf	,,	,,	,,	,,	160·1

From this experiment it would appear that the rate of flow of blood just drawn from the vessels of a living animal is very much greater than the rate of flow of blood which, having been defibrinated, has been allowed to stand for some time, as was the case in experiment 40. In defibrinated blood the corpuscles tend undoubtedly to run together, and the masses thus formed by their coherence must necessarily account for the extreme slowness. The pure and perfectly warm blood flowed, indeed, more rapidly than did the serum obtained from ox-blood, which had been used in a previous experiment. In experiments 36, 37, 38, and 39, it was found that the time of flow of equal quantities of serum and water were represented by the ratio of 1·4 : 1. In experiments 43—49, it was found, on the other hand, that the rate of flow of equal quantities of pure blood and water were represented by the ratio of 1·3 : 1.

In a former part of this paper we stated that the diameters of the tubes used by us differed from those of Poiseuille in being much wider.

As was previously stated, the French author found that in capillary tubes of different diameter, the quantity of fluid flowing in equal times through equal lengths, varies not as the squares, but as the fourth power of the diameters. In the tubes used by us, in the experiment above described, the diameter was such that the quantities of water flowing through equal lengths were, *cæteris paribus*, as the squares of the diameters. It is interesting to observe in connection with experiments 43—53 inclusive, that whilst the amount of water flowing varied very much as the squares of the diameters, the quantity of blood flowing through the two tubes did not obey this law; the blood being retarded in its flow more than water, though by no means to such an extent as to show that, for it, the tubes obeyed Poiseuille's law.

IV. *On the Pressure required to force Blood-Clot through Tubes of Narrow Diameter.*

The clot used was obtained by allowing ox's blood to coagulate, and separating it from serum.

Exp. 54.—In this experiment a tube having a diameter of 1·162 millimetre was used. Although subjected to the whole

atmospheric pressure (700 mm.) none of the clot would pass through the tube.

Exp. 55 *and* 56.—In this experiment the same clot was used, but a different tube. The clot was found freely to flow through the tube, which had a diameter of 2·00 millimetres.

In experiment 55 the pressure of a column of mercury 24 inches high was employed. In experiment 56 that of a column 29 inches high was required.

V. *On the Rate of Flow of Milk and Urine through Narrow Tubes.*

The results of these experiments are shown in the annexed table. It will be observed that two tubes were employed in the determination of the rate of flow of milk, whilst the two sets of experiments with urine were performed with one tube. The rate of flow of urine is shown to be almost identical with that of water, whilst the rate of flow of milk is about the same as that of water when a large tube is used, but much slower when a tube of narrow diameter is employed.

TUBE A.

Fluid used.	Diameter of Tube.	Length of Tube.	Temperature.	Pressure.	Time of Flow of 100 Cubic Cents. in Seconds.
	mm.	mm.		mm.	
Water . . .	·928	914	17°·C	601·97	69·2
Urine, Sp. Gr. 1018	"	"	17°·5	"	71·3
Urine, Sp. Gr. 1007	"	"	"	"	70·3
Cow's Milk . .	"	"	24°·6	594·3	90·3

TUBE C.

Fluid used.	Diameter of Tube.	Length of Tube.	Temperature.	Pressure.	Rate of Flow of 100 Cubic Cents. in Seconds.
	mm.	mm.		mm.	
Water . . .	1·259	914	15°	601·97	42·1
Cow's milk . .	"	"	27°	"	38·1
Goat's milk . .	"	"	22°	"	36·09

ON THE RELATION WHICH EXISTS BETWEEN THE IRON CONTAINED IN THE BILE, AND THE COLOURING MATTER OF BLOOD[1]. By P. A. Young, M.D. (*Edin.*).

The idea that the adult liver might act as a blood-destroying organ, has occurred to different physiologists, but has never been proved in a manner sufficient to warrant its being received as a recognised fact.

Of late years the chemistry of blood-colouring matter has been investigated with great care, and the quantity of iron entering into its composition determined with extreme accuracy.

On incinerating bile-solids the ash presents a reddish tinge, indicating the presence of iron, an appearance existing in the ash of no other animal fluid except blood.

The consideration of these facts has led to the present research.

In order to ascertain with certainty the relation between the colouring matter of blood and bile, it was considered necessary to make a careful analysis of the latter and to determine the exact quantity of iron present.

The mode of analysis was as follows. Human bile and that of the ox and of the dog were employed. A certain quantity of bile having been carefully weighed out was evaporated to dryness in a water-bath, then incinerated in a crucible to get the ash. The incombustible matter was dissolved in strong hydrochloric acid with the aid of heat, and then water was added. The fluid was now transferred to a small flask through the cork of which there passed a glass tube about nine inches long, bent in the middle to an angle of about 60°. A small piece of pure zinc was dropped into the fluid, and heat applied with the object of reducing the iron.

[1] This paper consists of extracts from a Thesis presented in 1870 to the Medical Faculty of the University of Edinburgh.

When the zinc was dissolved, which was shown by effervescence having ceased, the end of the tube leading from the flask was plunged into cold water, which rushed over into the flask and thus prevented the entrance of air during cooling. The contents of the flask were poured into a beaker containing water. The quantity of water used was the same in all the experiments. The iron was now estimated by the ordinary volumetric method, by means of a very weak standard solution of permanganate of potash. Sulphate of iron and ammonia was used to determine the strength of the solution, of which 1 cubic centimetre corresponded to ·00062 gramme of pure iron.

My first analyses were made with ox-bile. This was obtained from the gall-bladders of animals that had just been slaughtered, and was quite fresh.

Analysis I. 14·820 grammes of ox-bile were taken and evaporated to dryness in a water-bath. The solid residue weighed 1·112 grms. This residue was incinerated, and all organic matter burnt off. The ash weighed ·167 grms. This was dissolved in hydrochloric acid and triturated. It was found to contain 0·00093 grms. of Fe, being the quantity of iron in 14·8205 grms. of bile; or, in 100 grms. of the same bile there would be ·006207 grms. of pure iron according to the proportion 14·8205 grms. : 100 grms. :: ·00093 grms. : x.

Analysis II. 33·771 grms. of ox-bile were taken and evaporated to dryness, and incinerated in the same way as in the last experiment. The quantity of iron present was ·001736 grms., so that in 100 grms. of the same bile there would be ·005140 grms. of iron.

Analysis III. 39·738 grms. of ox-bile were evaporated to dryness, the residue weighed 3·139 grms. The ash left after incineration weighed ·361 grms. The bile contained ·00124 grms. of iron; or, in 100 grms. of the same bile there would be ·00312 grms. of iron.

Analysis IV. 26·353 grms. of ox-bile were used. The ash weighed ·224 grms. It contained ·000806 grms. of iron; or, in 100 grms. there would be ·00306 grms.

TABLE I.

Table showing the result of four analyses of ox-bile.

Quantity of bile used. grms.	Solids. grms.	Ash. grms.	Iron. grms.	Iron in 100 grms.
14·820	1·112	·167	·000938	·00620
33·771	"	"	·001736	·00514
39·738	3·139	·361	·00124	·00312
26·353	"	·224	·000806	·00306

It will be seen from the last column that the quantity of iron varies in the different specimens examined. Such variations must necessarily occur in all animal fluids, as many circumstances modify their composition. The mean of the last column of the table gives us ·004381 grms., which represents the quantity of iron in 100 grms. of ox-bile, according to four analyses.

On account of the difficulty of procuring dog's bile, I have been able to make only one analysis. As this is interesting I will give the result. 11·53605 grms. of bile from a dog just killed were evaporated to dryness, and found to contain 2·983 grms. of solid matter. On incineration the ash weighed ·267 grms. It contained 0·016 grms. of Fe.[1]

My next analyses were those of human bile, which I obtained through the courtesy of Dr Pettigrew from the gall-bladders of bodies examined in the Pathological Theatre of the Royal Infirmary.

Analysis I. 34·713 grms. of human bile were evaporated to dryness and then incinerated, the ash weighed ·442 grms. and contained 0·001705 grms. of Fe. In 100 grms. of the same bile there would be ·00491 grms. of pure iron.

Analysis II. In this case the bile was obtained from a man who had died after being operated on for strangulated hernia.

[1] Dr Gamgee informs me, that when incinerating dog's bile during his researches for the mercurial committee, both Dr Rutherford and he invariably noticed the decidedly red tinge of the ash, indicating the presence of iron in large quantity. This observation is confirmed by the present analysis.

All the organs were found on post-mortem examination to be quite healthy. 28·366 grms. were employed, and the quantity of iron was found to be ·00155 grms.; or, in 100 grms. of bile ·00546 grms. of iron.

Analysis III. 23·049 grms. of human bile were evaporated and incinerated and found to contain 0·00255 grms. of Fe; or, in 100 grms. of bile there would be ·0102 grms. of iron.

Analysis IV. 39·232 grms. of human bile contained ·00155 grms. of iron; or, in 100 grms. of bile ·00395 grms. of pure iron.

Analysis V. Woman from whose gall-bladder the bile was taken died of cerebral apoplexy. 35·982 grms. of bile were taken, and contained ·00155 grms.; or, in 100 grms. of bile ·004308 grms. of iron.

Analysis VI. This bile was obtained from the gall-bladder of a man who died of delirium tremens. 36·460 grms. of bile were found to contain ·00252 grms. of iron; or, in 100 grms. of the same bile there would be ·0115 grms. of pure iron.

TABLE II.

Table showing the result of six analyses of human bile.

No.	Quantity of bile. grms.	Iron. grms.	Iron in 100 grms.
1	34·713	.001705	·0049
2	28·366	·00155	·0054
3	23·049	·002356	·0102
4	39·323	·001550	·0039
5	35·982	·001550	·0043
6	36·460	·00252	·0115

If we take the average of these six analyses we find that the quantity of iron in 100 grammes of bile amounts to ·0065 grms. The bile employed in the second experiment was obtained from a man whose organs were quite healthy, and the

quantity of iron found in it is only one centigramme in excess of the average of the six experiments.

It will be seen from the above tables that the quantity of iron present in human bile is greater than in that of the ox, but less than in that of the dog. This is accounted for by the fact that dog's bile contains more solids, and ox-bile less, than that of the human subject.

The question now suggests itself, whence does the iron found in the bile proceed? The resemblance between the colouring matter of bile and a product of the alteration of blood-colouring matter was pointed out by Virchow in 1848, and physiological chemists have long speculated as to the exact relation which exists between bilirubin and hæmatin, a relation which is most probable both from the ultimate composition of these bodies, no less than from the fact that any substance which when introduced into the blood leads to the solution of blood-corpuscles, and therefore to the setting free of hæmoglobin (*e.g.* salts of the bile-acids), causes the appearance of bilirubin in the urine.

The demonstration that bile does contain very considerable quantities of iron appears remarkably to strengthen the view that the bile contains constituents derived from the blood-corpuscles, seeing that we can scarcely suppose that the iron is derived from any other substance than hæmoglobin, the only iron-containing proximate principle of the body as yet known to us.

Knowing as we now do that the quantity of iron in hæmoglobin is quite constant, viz. 0·42 per cent., we may calculate the amount of blood-colouring matter represented by the iron found in the bile, assuming that the latter is entirely derived from the former. Let a represent the quantity of iron in 100 parts of bile, and x the amount of blood-colouring matter represented by this quantity of iron, then

$$x = \frac{100 \times a}{0{\cdot}42}.$$

Thus in the first experiment we found 100 grammes of ox-bile tc contain 0·0062 grammes of pure iron, which corresponds, on the previously mentioned hypothesis, to 1·46 grammes of hæmoglobin.

TABLE III.

Shows the quantity of pure blood-colouring matter represented by the iron in 100 parts of bile.

No. of the Experiment.	Quantity of Iron in 100 grms. of Bile.	Quantity of Hæmoglobin corresponding to the Iron.
1	0·0062	1·46
2	0·0051	1·20
3	0·0031	0·74
4	0·0030	0·73

By taking the average of these four calculations it will be found that the quantity of pure blood-colouring matter (hæmoglobin) represented by the iron in 100 grammes of ox-bile is 1·032 grammes.

In the analysis of dog's bile I found that 100 grammes of bile contained 0·016 grms. of Fe, which corresponds to 3·81 grms. of hæmoglobin. The experiments of the Edinburgh Mercurial Committee of the British Association show that the average secretion of bile in the dog amounts to 111·17 grammes. Assuming the bile which was analyzed by me to be of average composition, it would follow that the average amount of iron daily excreted by the dog would correspond to the iron contained in rather more than four grammes of hæmoglobin.

In the following table is shown the quantity of blood-colouring matter represented by the iron in human bile.

TABLE IV.

No. of the Experiment.	Quantity of Iron in 100 grms. of Bile.	Hæmoglobin corresponding to the quantity of Fe found.
1	0·0049	1·17
2	0·0054	1·30
3	0·0102	2·43
4	0·0039	0·94
5	0·0043	1·02
6	0·0115	2·73

The average of these results gives 1·598 grms. as the quantity of pure blood-colouring matter represented by the average amount of iron found in my six analyses of human bile.

Carrying out further the assumption that the iron of the bile is entirely derived from hæmoglobin, let us try and ascertain in an approximate manner to what quantity of fresh blood-corpuscles this amount of iron would correspond.

I shall assume that 100 grammes of arterial blood contain 12·35 grammes of hæmoglobin, and that the same blood contains 51·304 grms. of moist blood-corpuscles (C. Schmidt); in other words, that the ratio of hæmoglobin to the blood-corpuscles is as 12·35 : 51·304.

Then the iron found in 100 grammes of ox-bile would represent 4·28 grms. of moistblood-corpuscles. Similarly, 100 grammes of the dog's bile examined by me contained iron which would correspond to 15·82 grammes of moist blood-corpuscles, and 100 grammes of human bile contained iron equivalent to 6·63 grammes of moist red blood-corpuscles.

From the observations related in this paper, taken in connection with other facts previously ascertained, the following conclusions may, I think, be legitimately arrived at.

I. That bile is a secretion rich in iron, and although the amount varies in different specimens, the variation is not greater than that of other characteristic constituents of that fluid.

II. That in all probability the iron in bile is derived from hæmoglobin, the only iron-containing proximate principle of the animal body.

III. That the presence of iron in the bile renders it most probable that the destruction of red blood-corpuscles takes place in the adult liver.

The above investigation was carried out under the direction, and in the laboratory of, Dr Arthur Gamgee, at Surgeons' Hall, Edinburgh.

NOTE ON DR YOUNG'S PAPER. By ARTHUR GAMGEE, M.D., F.R.S.E., *Lecturer on Physiology, Surgeons' Hall, Edinburgh.*

IT may appear to some that the hypothesis made by Dr Young, that the iron contained in the bile is derived from hæmoglobin, is unwarranted. Its agreement with other facts which are known to us, and which are mentioned by him, renders it, however, a probable hypothesis, which I believe admits of verification, even in the present state of science.

It is now essential that we should determine the amount of iron contained in liquor sanguinis with the greatest attainable accuracy. All authorities agree that this fluid contains only the minutest traces of iron. The following sentence contains the most definite statement I have yet been able to find on the subject. 'Bisweilen,' says Kühne, under the heading of Die Salze des Plasma's, 'die serum asche auch etwas CO_2, und Kieselsäure, und in hinläglich grossen quantitäten können auch spuren von Eisen, Mangan, und Kupfer nachgewiesen werden.' If the quantity of iron in liquor sanguinis, which can never be obtained absolutely free from blood-corpuscles, be as small as we should infer from this sentence, we should be forced to the conclusion that the iron of the bile is certainly derived from the iron-containing constituent of the blood-corpuscle, *i.e.* from hæmoglobin.

Many other researches now suggest themselves as likely to throw a light upon the subject of the transformation of the red blood-corpuscles, as for instance, a determination of the iron in all the different secretions, and a relative determination of the amount of iron and hæmoglobin contained in the tissue of the liver and spleen.

ON THE CRANIAL OSTEOLOGY OF POLYPTERUS.

By RAMSAY H. TRAQUAIR, M.D., *Edin.* *Professor of Zoology in the Royal College of Science, Dublin.* Plate VI.

(*Read before Section* D *of the British Association at Liverpool, September* 20, 1870.)

AGASSIZ described the cranium of Polypterus Bichir as being one of extreme simplicity, as regards the number of bones entering into its structure. Leaving out the plates roofing over the nasal cavities, and which he referred to the *ethmoid* of Cuvier, he only found two *frontals*, two *parietals*, two *mastoids*, one *basilar* and one *sphenoid*, entering into its composition as distinct and separable bones; the other osseous elements of the fish-skull being either wanting or united with those named[1]. And M. Duméril has also recently affirmed, that he has verified by his own researches the statements made by Agassiz on this point[2].

The cranium of Polypterus certainly contains a smaller number of distinct bones than that of ordinary Teleostei, but the list is nevertheless larger than that given by Agassiz. Twenty-seven years ago, Johannes Müller[3], besides proposing some alterations in the nomenclature of the facial bones of Polypterus, pointed out that its cranium possessed distinct pre- and post-frontals, as well as a separate median Cuvierian ethmoid, distinct from the plates designated as such by Agassiz. Müller's views as to the nomenclature of the bones of the head of Polypterus were further illustrated in one of the beautiful plates accompanying his well-known memoir on the Ganoids[4], presented a year later to the Academy of Sciences at Berlin.

Beyond the writings of Agassiz and Müller, I have seen no detailed account of the skull of Polypterus, although its structure is frequently incidentally referred to in handbooks, and

[1] *Poissons Fossiles*, T. II. 2e partie, p. 38.

[2] *Histoire naturelle des Poissons*, par A. Duméril, Paris, 1870, T. II. p. 374.

[3] 'Bericht über die Fortschritte der vergleichenden Anatomie der Wirbelthiere in Jahre 1842.' *Archiv für Anatomie*, 1843, s. ccxxxix.

[4] 'Ueber den Bau und die Grenzen der Ganoiden.' *Abhandlungen der K. Acad. der Wissenschaften.* Berlin, 1844 (1846).

other works on Comparative Anatomy and Palæontology[1]. I shall therefore proceed to give the results of my own observations on the structure of the cranium and face in Polypterus. My dissections were made on some small specimens of Polypterus Bichir, which formed part of the collection brought from the Nile some years ago, by Consul Petherick. They belong to a variety with few dorsal finlets (8—10) and much resembling the P. Senegallus of Cuvier; Dr Günther is however of opinion, that this and the other species of Polypterus hitherto recognised are but varieties of one comprehensive specific form, the P. Bichir of Geoffroy St Hilaire.

Primordial Cranium of Polypterus.

M. Duméril's statement is a very strong one, and, as well as the original description of Agassiz, can only be explained by supposing that, in old specimens, certain sutures may become obliterated. The specimens of Polypterus, which I have had an opportunity of examining, have been of rather small size, the largest of them measuring only $14\frac{3}{4}$ inches in length; but in all I have found the number of separable cranial bones to be greater than in the list given by Agassiz.

Fig. 2 represents the upper, and fig. 3 the lower aspect of the primordial cranium of Polypterus, prepared by the removal, after maceration, of the superficial or membrane-bones of the skull. Above, the cranial cavity is exposed by a large rectangular fontanelle, roofed over by the frontal bones (*Fr.* fig. 1), and below there is a smaller one (pituitary), covered up in like manner by the parasphenoid (x, fig. 5). Besides the ossifications which it includes, this primordial cranium consists very largely of cartilage, especially in the otic region behind, and in the nasal part in front. The cranial cavity commencing posteriorly at the foramen magnum (O. F.) is continued forwards between the orbits as far as the nasal chambers, where it terminates in two rounded openings, one into each of these. These spacious cavities, containing the complex olfactory apparatus described

[1] Huxley's 'Essay on the Classification of the Devonian Fishes,' *Mem. Geol. Survey*, Decade x. Lond. 1861. Owen's *Comparative Anatomy of the Vertebrata*, Vol. I. London, 1866. The Histology of Polypterus has been carefully examined by Leidig, *Zeitschrift für Wiss. Zoologie*, 1854.

by Müller and Leidig, are somewhat inflated in form, and mainly bounded by cartilage; each presents in front a rounded external opening, and they are separated from each other by a median vertical cartilaginous septum. Comparing this cranium with that of an ordinary Teleostean, it will at once be seen that the cartilage-bones entering into its formation are comparatively few in number; and, as observed by Leidig, they are mostly of a yellowish colour, whereas the superficial 'Deckknochen' are greyish white, and more compact in texture.

Posteriorly, there is but one occipital bone (*Occ.*), which completely surrounds the foramen magnum, thus occupying the place, not only of the basioccipital, but also of the exoccipitals: it is also very universally held to have incorporated with it the body of at least one of the vertebræ of the trunk, the first of the spinal series being represented only by a detached arch and pair of ribs. This occipital bone articulates behind with the arch of the first and the body of the second vertebra; below with the parasphenoid (*x*); laterally and in front it presents a deep notch on each side, completed by the opisthotic bone (*Op. o.*) into a foramen (8) for the passage of the vagus nerve; above it articulates, near its external angle, on each side with the parietal (*Pa.* fig. 1). In front it is likewise continuous, above and below, with the primordial cartilage.

In front of the occipital there is, on each side, a bone (*Op. o.*) which aids in the support of the otolithic sac, and is drilled by both the external and posterior semicircular canals of the ear. Its posterior margin shews a very deep notch, which, with the occipital, is completed as aforesaid into a foramen (8) for the vagus nerve; on its outer side, in front, it articulates with a double process of the parasphenoid (*x*, fig. 4), and by its superior external margin with the parietal bone. Its margins, below, in front, and above, are also continuous with the cranial cartilage. This bone, the 'mastoid' of Agassiz and Müller, I must identify as the *Opisthotic* of Huxley, but it is evident that it also includes his *Epiotic* element.

Between these bones and the post-frontal region the walls of the primordial cranium are entirely cartilaginous. Above, this cartilaginous portion of the cranial roof is encroached on by the great fontanelle, and is covered by the parietal bones;

below, the cartilage stretches uninterruptedly from the basilar portion of the occipital to the bone (*Sph.*) which I have designated as *sphenoid.* At the side, the cartilage protects the greater portion of the auditory organ, the otolithic sac causing a prominent oval bulging on the infero-lateral aspect, in which bulging the opisthotic bone also takes part. In front of the vestibule the cartilage is pierced by a foramen (*Fa.*) for the passage of the facial nerve; and again, just at the hinder margin of the bone *Sph.*, by two foramina for the second and third divisions of the trigeminal. There is thus no *Proötic* nor any *Pterotic* bone in the cranium of Polypterus; the region of the pterotic is partly covered by the lateral margin of the parietal, while the cartilaginous proötic region is strengthened by an anterior and posterior ascending process of the lateral wing of the parasphenoid (*x*, fig. 4).

Connected with the upper and external angle of the front of this cartilaginous otic region of the cranium, the *Postfrontal* bone (*Ptf.*) projects outwards and forwards, forming the prominent angle of the margin of the cranial roof behind the orbit. A small and delicate horizontal bar of cartilage passes forwards and inwards from near its tip, connecting it, as it were, by a little bridge, with the cartilage of the front part of the skull. The post-frontal bone consists of two portions,—a posterior, yellowish and spongy-looking, placed like a little vertical plate projecting down from the posterior external angle of the frontal bone; and an anterior part, white and compact, flattened horizontally, and closely applied to the lower surface of the frontal, along its external margin behind the orbit (see figures 4, 5, and 7). The post-frontal articulates by firm suture with the parietal, frontal, and anterior ascending process of the parasphenoid; posteriorly and internally it comes very close to the sphenoid (*Sph.*), but is still separated from it by cartilage. It is connected more loosely with the posterior suborbital bone (S. b.).

Between the otic portion of the cranium behind, and the nasal in front, extends the remarkable bone *Sph.*, lying between the frontal bones above, and the parasphenoid below, and enclosing that continuation of the cranial cavity which passes forwards between the orbits as far as the nose. It consists of two vertical laminæ which bound that prolongation of the cranial cavity

between them. The lower margins of those two laminæ are connected with each other behind by a narrow horizontal plate forming part of the floor of the cavity of the skull; and in front each vertical lamina again sends inwards from its lower margin a horizontal plate, which is however separated by a suture from its fellow of the opposite side. These last-mentioned horizontal plates are not visible from the under surface of the primordial cranium, being covered up by cartilage. Each vertical lamina, somewhat rectangular in shape, articulates above with the frontal of its side, in front with the prefrontal, below with the parasphenoid, and behind is continuous with the primordial cartilage. Its posterior margin is slightly notched by the foramina (5″ and 5‴), which transmit the 2nd and 3rd divisions of the 5th nerve; and near the middle of its lower margin is a foramen (2) for the optic nerve: above and behind the exit of the optic nerve the bone is also pierced by two small openings, one for the trochlearis nerve (4), the other (3) for the oculo-motor, abducens, and first branch of the trigeminal. The upper margins of the two vertical laminæ are largely separated from each other above by the great fontanelle, but in front of that opening they are connected with each other by a horizontal plate of cartilage continuous in front with that which roofs over the nasal chambers. The continuation of the cranial cavity enclosed by this bone is shut off from the nasal cavities by a cartilaginous partition in which two round holes are drilled, one for each olfactory nerve. Each of the principal laminæ, of which this bone consists, was in fact described by Agassiz, and alluded to by Müller, as a descending process of the frontal, from which however it is quite distinct.

It is evident, from its connections with surrounding bones and with the nerves issuing from this part of the cranium, that this bone (*Sph.*) occupies the entire space filled up in the skull of the Teleostean by the sphenoideum anterius and alæ orbitales of Cuvier behind, and the bone of the interorbital septum in front. In accordance with the nomenclature of Professor Huxley, who considers the interorbital bone to be the orbitosphenoid, and the sphenoideum anterius and orbital alæ of Cuvier to be the postsphenoid with its wings, I shall simply call this bone of Polypterus's skull 'sphenoid.' It is interesting to

observe, that if we were to imagine the portion of this bone behind the optic foramina cut off, the remainder united with the adjoining prefrontals (*Prf.*), and lastly ossification to invade the adjoining parts of the cartilage of the cranial roof and floor, as well as of the posterior part of the nasal septum—we should have a bone exactly the counterpart of the 'os en ceinture' of the frog's skull.

The general conformation of the nasal portion of the cranium has been already alluded to. Extensively cartilaginous, it contains three ossifications, two lateral, and one median. Each of the lateral ossifications (*Prf.*) has the form of a triangular plate, which, extensively connected with the primordial cartilage, enters into the formation of the anterior wall of the orbit, and the posterior part of the outer wall of each nasal chamber. It articulates by firm suture, behind with the interorbital portion of the sphenoid, by its apex with the frontal, and by its anterior margin with the premaxillary (*Pmx.*)—here it also comes into close relation with the anterior suborbital bone (*S.* b'). The lower margin articulates with the palate-bone (*Pl.*) by a joint which admits of a considerable amount of gliding motion. The apex of the bone is pierced by a foramen which transmits the ophthalmic part of the trigeminal nerve to the nasal cavity in front. Their connections render it evident that the two ossifications just described are the *prefrontal* bones.

The *ethmoid* (Cuvier), being the same bone called *nasal* by Owen and others, is a median ossification in the front of the septum narium, which projects like a little knob from the front of the primordial cranium (*E*, figs. 2 and 3). Above it sends backwards beneath the nasal bones (*Na.*) a flat narrow process, which is ossified in the membrane superficial to the cartilage. In the complete skull this median ethmoid is almost entirely covered up from view by other bones; a small lozenge-shaped portion is however seen on the front of the snout between the external nasal openings (see figs. 1 and 7, *E*). It articulates above and behind with the nasals (*Na.*), on each side with the two little bones marked *Na'*, in front and below with the premaxillaries. Below and behind it is touched by the anterior extremity of the parasphenoid, and on its lower or palatal

aspect a small lozenge-shaped portion, left exposed between the premaxillaries, is covered up by the anterior extremities of the vomers (*Vo.* fig. 5).

Membrane-bones of the cranium.

We shall now proceed to the description of those bones which may be stripped off from the macerated skull, without injury to the subjacent cartilage. Fig. 1 represents the cranium of Polypterus with membrane-bones, seen from above; fig. 4, seen from the side; and in fig. 5, by the removal of the facial bones of the left side, one-half of the cranium is also seen from below.

On the upper surface of the cranium, as seen in fig. 1, are three pairs of flat bones, readily removed after maceration; and which, in their relations to the cranium and to each other, at once recall to our minds the parietals, frontals and nasals of higher vertebrata. Their external surfaces are, for the greater part, without any covering of soft skin, are polished in aspect and beautifully tuberculated: and indeed, as is already well known, the structure of these bones, along with the opercular and other facial elements, in no essential manner differs from that of the osseous scales covering the body. Where they rest on the primordial cartilage, a thin layer of pigmented connective tissue intervenes, as has been pointed out by Leidig[1].

Each *parietal* bone (*Pa.*) is of a somewhat quadrilateral form, sending backwards a pointed process from its posterior margin near the external angle. Internally it articulates with its fellow of the opposite side, behind with the supratemporals (*St.*), in front with the frontal, and at its posterior external angle with the first post-spiracular ossicle. The external margin is beveled off downwards, and bounds the spiracle internally; it is in contact with the two spiracular ossicles, when the valvular opening of the spiracle is closed; behind the spiracle this outer margin helps also to form the articular cavity, in which the head of the hyomandibular bone (*H. M.*) moves. Each parietal rests below on the primordial cartilage, and articulates besides with the opisthotic (*Op. O.*), the occipital (*Occ.*), and the postfrontal (*Ptf.*).

The *frontal* bones (*Fr.*) articulate in the middle line with

[1] *Op. cit.* p. 49.

each other, behind with the parietals, and in front with the nasals and slightly with the premaxillaries: while the outer border of each forms the upper margin of the orbit, and, behind that, articulates with the pre-spiracular and the anterior one of the spiracular ossicles. On its deep surface each frontal articulates at its anterior external angle with the prefrontal; behind that, and for nearly the whole length of the bone, with the lateral perpendicular lamina of the sphenoid; and lastly, the post-frontal fits on below the posterior part of the external margin. Internally to the perpendicular laminæ of the sphenoid, the frontals rest on primordial cartilage, and roof over the great fontanelle; externally to the sphenoid they cover the orbits.

In advance of the frontals there are again two smaller and nearly square-shaped plates (*Na.*), covering the cartilaginous roof of the nasal chambers. Each articulates in the middle line with its fellow, behind with the frontal, externally with the premaxillary. The anterior border forms part of the superior margin of the external bony nasal opening; and, internal to that opening, articulates with the small ossicle (*Na'*), and touches the median ethmoid. I follow Müller and others in calling these two bones *nasals;* it will be seen that they occupy the same relative positions, as regards the nasal cavities and the cranial bones generally, as the bones called nasal in the frog. When Professor Owen, however, says that in Polypterus "the nasal is divided at the median line[1]," it is clear that he has overlooked the median bone (*E*), also present in Polypterus, and which certainly is the 'nasal' of his nomenclature. It is also clear, that if Owen's 'nasal' in the fish be the homologue of the 'nasals' in other vertebrata, some other signification must be found for the plates (*Na.*) just described. But if they are, to use Mr E. R. Lankester's terms, either homogeneous or homoplastic with any bones in the skull of higher vertebrata, I only conceive them to be the equivalents of the nasals.

In front of each nasal, near its anterior internal angle, and articulating both with it and the median ethmoid, there is, in every skull of Polypterus I have examined a small bony

[1] *Comp Anat. of the Vertebrata,* I. 114.

plate (*Na'*), which also contributes to the formation of the upper osseous margin of the external nasal opening. What signification can be attached to this ossicle is not at all clear to my mind; there is, without it, a distinct and moveable os terminale (turbinal of Owen) on the inner margin of the nasal opening (*O. t.* fig. 7).

Extending along almost the whole length of the base of the cranium is the largely developed *parasphenoid* (*x*, figs. 4 and 5). Cleft posteriorly, and ending in front in a sort of spoon-shaped extremity, it gives off on each side, a little behind the middle, a lateral process or wing. The elongated median portion, or body of the bone, covers up the inferior fontanelle, and a considerable portion of the primordial cartilage, both in front and behind; it articulates above also with the occipital and sphenoid, and by its anterior extremity with the premaxillary and the median ethmoid. In front of the lateral wings its palatal surface is also in contact with the inner edge of the entopterygoid (*Ept.*) on each side. Each lateral wing divides into three processes,—an anterior, posterior, and inferior. The inferior process, to which the anterior branchial arch is attached, extends nearly horizontally outwards and a little backwards, ending in a sharp point: the anterior one passes obliquely upwards and forwards over the primordial cartilage behind the exit of the second and third divisions of the trigeminal nerve to join the post-frontal; while the posterior process, dividing into two branches which enclose the exit of the facial nerve (*Fa.* fig. 4) between them, passes in like manner over the cartilage to the opisthotic bone. The anterior spoon-shaped extremity of the bone is covered on its palatal surface by numerous minute thickly-set teeth, which pass backwards in a narrow dentary band which bifurcates posteriorly, one branch passing on to the under surface of each lateral wing (see fig. 5).

A complex morphological importance has been assigned to this bone *x* in the skull of Polypterus. The anterior spoon-shaped dentary portion has been considered as an equivalent of the vomer, and the parts of the lateral wings which ascend anteriorly and posteriorly on the side of the cranium have been held to be the homologues of the alisphenoids and alæ orbi-

tales of Cuvier (the proötics and alisphenoids of Huxley). For my own part, I cannot see any reason to attach to this bone a significance beyond that of the parasphenoid of any other fish, or of an amphibian. As I shall presently point out, Polypterus possesses a double vomer in addition to its parasphenoid; the proötic is simply wanting; the alisphenoid is represented by a portion of the great bone *Sph.*, and the ascending processes in question I can only look upon as offshoots of the great 'splint-bone' of the base of the skull.

In front of the anterior extremity of the parasphenoid and internal to the dentary margin of the premaxillary (*Pmx.*) and maxillary (*Mx.*) are two narrow bones (*Vo.*), which meet each other in the middle line in front, and form together a figure like a horse-shoe. Only one of these bones, that of the right side, is represented in fig. 5, its fellow being removed. Each of them is in contact above with the median ethmoid (*E.*), with the premaxillary, and with the cartilaginous floor of the nasal chamber; by the greater part of its outer edge it articulates by suture with the maxillary bone, while its posterior extremity is articulated with the ectopterygoid (*Ecpt.*), coming also into close contiguity with the palate-bone and prefrontal. Its articulation with the maxillary and ectopterygoid bones is so firm, and its other connections so comparatively loose, that it shares, with its fellow, all the movements of the facial apparatus on the cranium. By Agassiz this bone was supposed to be a portion of the superior maxillary, by Müller of the 'palate-bone' (under which designation he included also the ectopterygoid), and hence the latter distinguished author wrote of the palate-bones of Polypterus as meeting each other in the middle line in front. But the bone (*Vo.*), of each side, is certainly distinct from any other; and the position of the two as membrane-bones on the lower aspect of the anterior part of the cranium, immediately behind the premaxillaries, and in advance of the parasphenoid, determines them, I think, to be the duplex *vomer*. Their determination as such certainly concurs, with other things, in giving a somewhat amphibian aspect to the anterior region of the skull of the Polypterine fishes.

Palato-suspensory Apparatus of Polypterus.

The palato-suspensory apparatus is similar in structure and connections to that in most Teleostei, save that the symplectic is absent, the hyomandibular possesses a small accessory piece at its upper extremity, and the ectopterygoid plate is so largely developed as to close out the comparatively minute palate-bone from view on the oral surface of the hard palate.

The *hyomandibular* (*H. M.*, fig. 6) is a narrow, elongated, laterally compressed bone, the long axis of which passes at first obliquely downwards and backwards from its articulation with the side of the cranium, and then a little above the middle of the bone bends forwards at nearly a right angle towards the articulation of the lower jaw. Opposite the posteriorly directed angle of curvature, a small process, tipped with cartilage, projects upwards and backwards to articulate with the operculum (*Op.*). The expanded upper extremity of the bone, furnished with an oval convex tip of cartilage, fits into an obliquely placed articular cavity on the side of the cranium, just behind the region of the spiracle, and which cavity is formed by the opisthotic and parietal bones and a portion of the cranial primordial cartilage. A suture is seen cutting off a triangular portion (*H. M'.*) from the posterior part of this articular head; this is the *accessory hyomandibular*, already referred to. The lower extremity of the hyomandibular pointing downwards and forwards is also tipped with cartilage, and is bound by ligament to the stylohyoid element; in front it is also somewhat loosely bound by connective tissue to the posterior part of the palato-quadrate arrangement.

This palato-quadrate arrangement is essentially the same as in Teleostei, both as to number of bones, and as to the relations of these to their axial lamina of cartilage. Behind there are two bones ossified from that cartilage, of which the lower and outer one (*Qu.*, figs. 5 and 6), presenting a transverse articular ridge for the lower jaw, is evidently the *quadrate* bone of ordinary fishes.

Müller correctly pointed out that Agassiz was wrong in identifying this bone with the 'transverse' of Cuvier. But Müller was wrong and Agassiz right, as regards the other bone

(*Mpt.*), which lies above, and to the inner side of the quadrate, and is extensively seen on the oral surface of the palate (see fig. 5). This bone, marked 'external pterygoid' by Müller in the figures accompanying his Memoir on the Ganoids in the Berlin Transactions, is undoubtedly the 'tympanic' of Cuvier, the 'pretympanic' of Owen, the '*metapterygoid*' of Prof. Huxley's nomenclature.

From the quadrate and metapterygoid bones behind a thin lamina of cartilage extends forwards on the upper surfaces of the two bony plates *Ept.* and *Ecpt.*, and ends in front in a small ossicle (*Pl.* fig. 6), which is moveably articulated to the lower margin of the projecting anterior inferior angle of the prefrontal. This little ossicle, seen in the front of the floor of the orbit, is all which represents the *palate-bone* in Polypterus; it is excluded from appearing on the oral surface of the hard palate by the bone (*Ecpt.*) presently to be described.

Connected with the under surface of the palato-quadrate lamina of cartilage are two elongated laminar membrane-bones (*Ept.* and *Ecpt.* figs. 5 and 6), forming the greater part of the hard palate on each side. The internal of these two (*Ept.*), whose inner margin glides on the parasphenoid, and which articulates by its outer margin with the metapterygoid and the bone *Ecpt.*, has always been correctly recognised as the pterygoid or *entopterygoid* bone. Its palatal surface is roughened by very minute teeth. The other externally placed bone (*Ecpt.*) articulates behind with the quadrate and metapterygoid, internally with the entopterygoid, in front with the vomer, and on the upper surface of its anterior extremity with the palate-bone, which separates it here from the prefrontal. A little in front of the middle of its external margin it sends outwards a small pointed process to join the superior maxillary (*Mx.*), and then, a small space intervening, it again comes into contact with the same bone on the outer margin of its anterior extremity. The palatal aspect of the bone is in front covered with minute teeth, a distinct and separate row of which, larger than the others, garnishes the anterior part of the external margin. From its position on the outer margin of the hard palate and its relations to the cartilage and to the other bones of the palato-quadrate system, it is evident that this is the *Ectopterygoid*,

the 'transverse' of Cuvier; although, by both Agassiz and Müller, it was described as 'palate-bone.'

The Jaws.

The *Premaxillary* bones, developed on the anterior-inferior margin and external surface of the cartilaginous nasal portion of the cranium, are immoveably articulated with the front of the skull. Each premaxillary (*Pmx.*), articulating in the median line with its fellow, sends backwards on the palatal surface of the nasal chamber, above the anterior extremity of the vomer, a small horizontal plate to join the end of the parasphenoid. Between these two little horizontal plates a small lozenge-shaped portion of the ethmoid (*E.* fig. 5) is exposed, which is however covered up when the vomers are in situ. On the upper surface of the snout the premaxillary is seen likewise to articulate with the median ethmoid (*E.* fig. 1), and with the little bone *Na'*. It then passes as a narrow bar round the inferior margin of the external nasal opening, to expand behind it into a large process passing upwards and backwards, and which articulates above with the nasal bone, behind with the prefrontal, and by its apex with the anterior external angle of the frontal. Below and behind the nasal opening the premaxillary is connected with the maxillary (*Mx.*) and with the anterior suborbital (*S. b'.*) by articulation admitting of considerable movement; above the latter bone it contributes to form the anterior margin of the orbit.

The *maxillary* bone (*Mx.* figs. 5 and 7) forms the greater part of the external dentary margin of the upper jaw; and its upper edge is also excavated to form the lower margin of the orbit. Behind the orbit it articulates with the posterior suborbital bone (*S. b.*) and with the other dermal bones (y, y', y'') covering the cheek; in front of the orbit with the anterior suborbital (*S. b'.*) and with the premaxillary; internally it articulates with the vomer and with the ectopterygoid. Its oral margin, as well as that of the premaxillary, is garnished with slender conical teeth, most of which are anchylosed with the bone.

The lower jaw consists, as is well known, of four pieces on each side: the articular (*Ar.* fig. 7), the angular (*An.*), the

dentary (*D.*), and the splenial (*Sp.*). The dentary bone bears the outer range of large teeth; the splenial lying along the inner aspect of the jaw covers up Meckel's cartilage, which extends from the articular element forwards to the symphysis, in a groove formed by the angular and dentary elements. The splenial likewise forms the upwardly directed coronoid process (the only portion of this element seen in fig. 7), and in front of that its upper edge is garnished with a range of closely set brush-teeth.

Passing over the *labial cartilage*, the *hyoid*, the *branchiostegal* plates, and the *branchial* bones, which are already very sufficiently known, we proceed to the

Opercular apparatus, and Dermal Bones of the head of Polypterus.

The *operculum* (*Op.*) and the *suboperculum* (*S. Op.*) are easily recognised; there is no *interoperculum*, and the presence of anything corresponding to the *preoperculum* is somewhat doubtful. The bone (*y*, fig. 7) lettered as such in the figures of Agassiz and of Müller is a large, irregularly shaped osseous plate covering the cheek and muscles of mastication, and extending from the operculum and suboperculum forwards as far as the superior maxillary bone. Posteriorly it is immoveably fixed to the hyomandibular and quadrate bones; but its connection with the operculum and suboperculum admits of course of free movement of the latter. Its upper anterior and lower margins are in contact with other dermal bones to be presently described. The greater part of its external surface is, like that of so many of the bones of the head, polished and granulated, save a small portion in front and a larger space below and behind, which are covered by soft skin. From the manner in which this plate extends forwards over the cheek, Prof. Huxley considers that it cannot be a preoperculum; and indeed is inclined to doubt the presence of a true preoperculum in any of the Crossopterygian Ganoids[1]. Agassiz himself did not consider the entire bone as corresponding to the preoperculum of other fishes, but only the posterior-inferior non-enamelled part, the

[1] *Classification of Devonian Fishes*, p. 40.

rest being comparable with the cuirass of polygonal plates covering the cheek in Lepidosteus[1].

In front of the plate last described and behind the posterior suborbital bone there is a small plate (y') covered with soft integument like the adjoining maxillary and suborbital bones. And again, along its lower margin there are, in typical specimens of P. Bichir, two, sometimes three, little plates with enamelled tuberculated surfaces; in the present variety however there is only one in front (y''), and that generally not enamelled.

The *spiracular* ossicles (*Sp. O.*) are two small plates, one of which, the posterior, is somewhat rectangular in form, while the anterior one is triangular, with its apex received into a notch on the posterior external angle of the frontal bone. These two ossicles form a valve protecting the spiracular cleft, which lies between their inner margin and the parietal bone. Behind the spiracular plates, and extending backwards in a row along the upper margin of the operculum are, in the specimen represented in fig. 7, four *post-spiracular* ossicles (z, z, z, z); and again in front of the spiracle there are two *pre-spiracular* plates (z', z') extending downwards and forwards towards the orbit between the frontal and postfrontal bones, and the great cheek-plate y. The whole of this antero-posterior chain of dermal bones have their external surfaces beautifully enamelled, and except in the case of those forming the spiracular valve, which are limited to two, their number is very irregular and is often different on the two sides of the head. In large typical specimens of P. Bichir the number of pre-spiracular ossicles especially is always greater than in the comparatively short-headed variety here figured.

The head of Polypterus possesses also the same sets of dermal bones, which in most Teleostei are so constantly found in connection with the cephalic ramifications of the slime-canal system. The first of these sets (*S. t.*) corresponds exactly with the *supratemporal* range of ordinary osseous fishes, and exactly as in the salmon conducts a transverse commissure of the lateral canal-system across the back of the head. There are always six supratemporals in Polypterus; the first on each side lies in front of the post-temporal[2] and above the post-spiracular bones,

[1] *Poissons Fossiles*, II. part 2, p. 40.

[2] *Post-temporal* (Parker); *supra-scapular* (Cuvier); upper *supra-clavicular* (Gegenbaur).

the other two with their fellows form a transverse chain across the back of the head immediately behind the parietal bones. Behind the median pair the proper scales of the trunk commence.

The chain of *suborbital* bones is interrupted in the middle, there being only two present, an anterior and a posterior, while the lower margin of the orbit is formed by the superior maxillary bone. The *posterior suborbital* (*S. b.*) passes downwards and forwards from the postfrontal to the maxillary bone; and the *anterior* (*S. b'.*) fills up the angle between the maxillary and premaxillary in front of the orbit,—internally it is also connected with the prefrontal. Both suborbital bones are covered by soft skin, and are drilled by the suborbital slime canal.

The *os terminale*[1] is a very small, curved, tubular ossicle (*O. t.* fig. 7) connected with the termination of the main lateral mucous canal of each side. Covered with soft skin, it is loosely and moveably placed on the superior internal margin of the external nasal opening of the cranium (*Pl.*). The small immoveable plate *Na'.*, which lies to its inner side, looks almost like a reduplication of the os terminale.

Distribution of the Slime-canals on the head of Polypterus.

The ramifications of the slime-canal system on the head of Polypterus follow a plan essentially similar to that which obtains in osseous fishes. The *main* lateral canal of each side penetrates the post-temporal bone, and then is conducted, through the first and second supratemporal ossicles, to the parietal. Its course forwards lies then through the parietal, postfrontal, frontal, nasal, and accessory-nasal bones, until it terminates in front in a transverse commissure, which, passing across through the ethmoid, joins it with the extremity of the canal of the opposite side. On its way it gives off, first a *supratemporal* branch, which, passing across the back of the head through the supratemporal bones, forms a posterior transverse commissure with the system of the other side. The *suborbital* branch, leaving the main canal in the postfrontal bone, passes downwards and forwards, round the posterior and inferior mar-

[1] *Os terminale* (Stannius); *os nasale* (Cuvier); *turbinal* (Owen).

gins of the orbit, through the postfrontal, posterior suborbital, superior maxillary and anterior suborbital bones, and then entering the premaxillary, it proceeds forwards lodged in that bone beneath the lower margin of the nasal opening, and terminates at last by joining the anterior transverse commissure in the ethmoid. From the point, where the main and suborbital canals meet in front, a small branch is given off upwards and backwards, lodged in the moveable os terminale. The operculo-mandibular canal, distinct from the main canal and its branches, drills the hinder border of the great cheek-plate (*y*), just as the corresponding one in Teleostei drills or grooves the preoperculum. It then enters the angular piece of the lower jaw, and proceeds forwards through it and the dentary to the symphysis, where it forms a junction with its fellow of the opposite side. Numerous pores, symmetrically placed on the two sides of the head, connect these canals with the external surface; the points where these pierce the bones are indicated in figures 1 and 7. There is a median pore at the symphysis of the jaw, where the operculo-mandibular canals of the two sides join each other.

EXPLANATION OF PLATE VI.

The lettering (nearly the same as that employed by Professor Huxley in his *Atlas of Comparative Osteology*) is uniform throughout all the figures.

An. Angular.
Ar. Articular.
D. Dentary.
E. Ethmoid.
Ecpt. Ectopterygoid.
Ept. Entopterygoid.
Fa. Exit of facial nerve.
Fr. Frontal.
H. M. Hyomandibular.
H. M'. Accessory Hyomandibular.
Mpt. Metapterygoid.
Na. Nasal.
Na'. Accessory Nasal.
Occ. Occipital.
O. F. Occipital foramen.
Ol. Nasal opening.
Op. Operculum.
Op. O. Opisthotic.
O. t. Os terminale.
Pa. Parietal.
Pl. Palate bone.
Pmx. Premaxillary.
Prf. Prefrontal.
P. t. Post-temporal.
Ptf. Postfrontal.
Qu. Quadrate.
S. b. Posterior suborbital.

S. b'. Anterior suborbital.
Sp. Splenial.
Sph. Sphenoid.
Sp. O. Spiracular ossicles.
St. Supratemporals.
Vo. Vomer.
x. Parasphenoid.
y, *y'*, *y''*. Dermal bones on the cheek.
z, *z*, *z*, *z*. Postspiracular ossicles.
z', *z'*. Prespiracular ossicles.

2. Exit of optic nerve.
3. Foramen for the passage of the oculomotor, abducens, and first division of the trigeminal nerves.
4. Exit of trochlear nerve.

5''. Exit of 2nd division of trigeminal.
5'''. Exit of 3rd division of trigeminal.

In these figures the cartilage is indicated by fine dotting. In fig. 5, however, care must be taken not to confound the dotting indicating the minute teeth on the parasphenoid, entopterygoid, and ectopterygoid bones, with that which marks the cartilage in other parts of the figure.

Fig. 1. Upper surface of the cranium of Polypterus, with the membrane bones in situ. Enlarged one-third.

Fig. 2. Upper aspect of the primordial cranium of Polypterus, the membrane-bones stripped off. From a smaller specimen, and magnified two diameters.

Fig. 3. The same primordial cranium with its ossification seen from below.

Fig. 4. Lateral view of the cranium represented in fig. 1.

Fig. 5. Skull of Polypterus, seen from below; the maxillary bone, the vomer, the palatal, suspensory, and opercular apparatus being removed from the left side, and the lower jaw and branchial apparatus from both. On the right side of the head the upper jaw, vomer, palato-quadrate, and opercular apparatus are seen. Enlarged about one-third.

Fig. 6. Opercular bones, hyomandibular, and palato-quadrate apparatus of the left side of the specimen represented in fig. 5, seen from the outer side. The vomer is retained attached to the anterior extremity of the ectopterygoid.

Fig. 7. The same skull represented in fig. 5, but with the lower jaw still attached and seen from the right side.

REVIEWS AND NOTICES OF BOOKS.

Contribution to the theory of Natural Selection. By ALFRED RUSSEL WALLACE. Macmillan and Co., 1870.

THE series of Essays collected into one volume, under the above title, by the sagacious naturalist of the Malay Archipelago, has, with the exception of one Essay, been already published in various periodicals. The vein of thought which runs through them is so original and characteristic of the author, that it is well that they should be given to the public in this form, although some of these Essays seem to have little to do with the 'theory' which has of late occupied so much of the attention of naturalists; and the concluding ones raise a grave exception to the universal application of natural selection in the evolution of organisms. It is a rare luxury to read the works of a man who goes so directly to nature for his facts, and to his own experience for illustrations; who is so little trammelled by the preconception of others, and whose acquaintance with his subject is so wide and so minute that his speculations are always noteworthy if not convincing. In keenness of observation, in logical power, in courage, and in candour, Mr Wallace is only second to the author of the Origin of Species, who, coming after him, Mr Wallace is content should be preferred before him. By his clearness and vigour of thought and expression, he has succeeded in presenting the results of great labour in such a manner as to abstract all labour from the reception of them.

The first three Essays have already attracted a large amount of public attention. The first two mainly because Mr Darwin accredits the author with having arrived at almost exactly the same conclusions as himself on the origin of species; and the latter on 'mimicry,' because it was published in so popular a form and in so widely-read a publication as the *Westminster Review;* and also because of the great interest of the subject, and the intrinsic merit of the manner in which it was treated. It may safely be predicted that the study of the subject of 'Mimicry' will reveal a multitude of interesting and suggestive facts from fields already explored, as well as from every fresh hunting-ground of the naturalist; and that Mr Wallace's clear and masterly article will always be recognized as having furnished the impetus which gave direction to this line of enquiry.

In estimating the share which Messrs. Darwin and Wallace have respectively taken in enunciating the 'theory of Natural Selection,' we must exercise our own judgments, as each of these distinguished men manifests a generous desire to attribute to the other more than his due. The priority of publication is clearly with Mr Wallace. It was the second of these Essays, which Mr Darwin tells us precipitated the publication of his best-known work; although there is little evidence of precipitation to be found in its pages. The general

proposition that "every species has come into existence coincident both in time and space with a pre-existing closely allied species," which is the Q. E. D. of the first essay, coupled with "the tendency of varieties to depart indefinitely from the original type," which is the subject of the second, points directly to the derivative origin of species. In these two Essays the author may be said to have traced the chain of evolution from either end until the investigations overlap each other and have passed the point of junction. Nevertheless he has nowhere boldly grasped the idea of derivative origin—that idea which has proved such a nettle in the hands of others. His omission to do this when the facts seemed to point so directly to that conclusion, will be looked upon by some as a want of courage, and by others as an exhibition of commendable scientific caution. As, however, the only original and most valuable part of Darwin's work consists, not in the demonstration that variation may be exaggerated until it amounts to a specific distinction, but in the method by which this is accomplished; and this *modus operandi* is certainly distinctly sketched out by Wallace in lines of thought so parallel with those of the authors of the Origin of Species, as to have prompted an absolute identity in many of the phrases employed by both; we think that no insignificant share in the honour which attaches to the grand generalization belongs to the author whose merits we are now discussing.

We need scarcely make any comment on the 'Malayan Papilionidæ.' In this Essay the author occupies ground which is peculiarly his own. He thinks the study of this family of special value as applied to illustrate natural selection. His treatment of the subject, including the wonderful phenomena of polymorphism, dimorphism, and mimicry, is very similar and not a whit inferior to F. Müller's discussion of the wider group of crustacea in reference to the same theory.

The short chapter on instinct is admirably clear and sagacious; and his deduction that the capability of the savage to travel unerringly through unknown and trackless forests, is due to intelligence and not to instinct, is wholly convincing. Probably, however, thoughtful and scientific men have never thought otherwise. That the faculties of observation and memory may perform wonders by being long and almost exclusively directed to accomplish one end, is quite as well exhibited by the facts of the conjuror whose disciplined eye can detect in one instantaneous glance accurately and distinctly enough to inform the memory, the nature of a whole hand of cards, as by the savage at home in the wilderness. Both feats are no doubt due to education, which is exactly the element which is wanting in all instinctive acts. How this Essay on instinct is connected with natural selection may be traced in the two succeeding ones. In the first of these there is a very ingenious comparison between the construction of his habitation by man and the nesting of birds. An attempt is made to represent these as precisely parallel acts by attributing the building habit in man to the exercise of a lower faculty than reason, while in the bird it is referred to a higher mental power than instinct. Mr Wallace thinks the imitative habit, de-

pendent as it is on observation, memory, and that limited amount of reason which is exhibited both by animals and the human race, is sufficient to account for all habitable structures from the Doric temple to the rook's nest. We do not doubt that there is more analogy between the human and aërial architect than a *primâ facie* view of the case would indicate; but surely there is an *analogy* and not an *identity*. Although our instinctive acts are so dominated by our reason that they are with difficulty detected and circumscribed, yet we are conscious of both instinctive and intelligent acts. The building of our houses is not an instinctive act, while the nidification of birds certainly does appear to be the "performance of a complex act absolutely without instruction or previously acquired knowledge," which is Mr Wallace's own definition of instinct. Mr Wallace disputes the latter part of this assertion; and before offering a few remarks in support of it, it is only fair to admit that he has brought forward some striking facts and analogies which clearly strengthen his view of the case. We certainly should have supposed that the song of the bird was an instinctive, but experience proves it to be an imitative act. That birds alter their nests when altered conditions require it, is made familiar to us by the very various nests constructed by the house-sparrow, and this cannot be referred to an altered instinct, because these various nests are constructed at the same period by the same species. Mr Wallace also points out that his theory is capable of disproof, and challenges the experiment. "No one," says he, "has ever yet obtained the eggs of some bird which builds an elaborate nest, hatched these eggs by steam or under a quite distinct parent, placed them afterwards in an extensive aviary or covered garden, where the situation and the materials of a nest similar to that of the parent birds may be found, and then seen what kind of nest these birds would build." In a former Essay he writes he expects facts alone to be brought to disprove his theory, not *à priori* arguments against its probability. We sincerely trust some enthusiastic young naturalist will make the *experimentum crucis* indicated, but in the meantime does not nature supply us with a fact? Why does not the cuckoo before laying her eggs construct an exact facsimile of the hedge-sparrow's nest in which she was reared? The author seems to forget that his own theory is positive, and the converse experiment is necessary to complete its proof, and a "point which can be proved should not be assumed." It is true this might be allowed if instinct were a "totally unknown power," and unreasoning imitation a thoroughly explained propensity. The author however does not absolutely deny the existence of instinct. Indeed! he has defined and illustrated it. Instinct is therefore a *vera causa*, and unreasoning imitation is an unexplained phenomenon. Thus the question recurs; why do birds build a nest at all? Why does not the imitative propensity come into play at once and always, instead of at the period exactly before parturition? How is it that a bird builds without assay or failure! Why does she not build mimic nests as children construct grottoes and houses of cards? In man the inducement to build is personal and pressing. Sharp winds, drenching

rain, and scorching suns—to say nothing of unprotected property, and the person exposed to night attacks from enemies—are present evils which appeal to his reason for a prompt remedy. The feathered bird has no care for herself or the fragile egg she yet carries, unless she possess some faculty far other than those which prompt to purely imitative acts. If the author's theory be the true one, a bird must have the faculties necessary to correct imitation in far higher degree than man. How many of us, though we have lived in houses all our lives, could construct a habitation, even though we had the materials ready to hand, which would keep out rain or not fall in less than a twelvemonth, if we did not consult a professional builder? This is not because of the complexity of the structure. A child taken from a hut of wattle and daub could not construct a like one unless he was shown how to do so; or if by returning and examining it and pulling it to pieces he should accomplish this feat, it would be by the exercise of the reasoning powers. It may be safely stated that man builds his habitation by imitation far more than the bird. If he had to examine his dwelling and think for himself how each part was constructed and put together, the process would be laborious in the extreme; but, in fact, before he begins to build he not only looks on while other houses are built, but he is instructed how to put each part together. On the other hand, the bird has never seen its cradle woven. If it builds one like it, strictly speaking it is not an imitative act at all, but a series of deductive reasonings and constructive acts. Again, if the bird were bent on repeating a number of acts which it has never witnessed but only derived from a study of the results which lie around it, how could it distinguish which part of the structure it must supply and which part must be looked for ready made. The house-martin, unless it had a better estimate of the limits of its powers than young persons usually have, would begin to build the house before it placed the nest under the eaves. Why do not the American wren and purple martin, which are so constantly reared in the small cigar-boxes furnished by their kindly hosts, begin at the beginning, and frame, or endeavour to frame, the box first? Under this supposition, the European stork would become by this time an accomplished wheelwright, since it is the custom of farmers to place wheels in their outbuildings to lodge the nests of these birds. Of course these illustrations are extravagant, but they best exhibit the difficulty that according to this theory the bird has at the very outset of its undertaking to go through two processes of ratiocination, and determine first that the nest in which it was reared was made, and secondly that it can make one like it. The explanation of the above difficulties is to be found in the fact that man is prompted to build by reason, and builds as he does by imitation, while the birds build and are compelled to build by an impulse which is more akin to reflex action of the nervous system than to those acts which involve intelligence and will. We have some remnants of this strange impulse revealed to us by our own consciousness, and it is therefore not illogical to attribute the acts of animals which admit of no other explanation to this true cause. It

is singular that the province of instinct should be represented as so narrow and circumscribed by an advocate of the theory of natural selection; for the existence of habits, not based on any reasoning process, which nevertheless adapt their possessors admirably to existing, but not permanent, circumstances is one of the phenomena which are best explained by this theory.

The next Essay is devoted to the proof of the law that when both sexes of birds are of strikingly gay and conspicuous colours, the nest is such as to conceal the sitting bird; while, whenever there is a marked contrast of colours, the male being gay and conspicuous, the female dull and obscure, the nest is open and the sitting bird exposed to view. Notwithstanding the numerous exceptions to this law, which are very candidly presented, its existence as a rule or generalized truth must be admitted. Natural selection may operate in making the male bird conspicuous. It must, if it have any operation at all, tend to render the female who sits on an open nest inconspicuous. But we are brought by these considerations face to face with the problem of the beauty of organisms. The facts teach us that in the class Aves and the order Lepidoptera,—the two divisions which have most occupied the attention of the author,—there is a strong tendency, a *nisus*, towards the development of colours so varied, contrasted, and arranged into patterns, as to create in us the pleasurable sensation whose exciting cause we call beauty. The colour, contrasts, and patterns are so disconnected from the structures which lie beneath and so independent of the subjective vital functions, that they offer the best instances of beauty pure and simple (*i.e.*), beauty severed from use or advantage. The fitness which natural selection spares, as a sculptor leaves the statue by clipping away the remainder of the block, can only relate to concealment and conspicuousness. All must agree with Mr Wallace that concealment is a sufficient explanation of the plainness of the otherwise unprotected female; but will the advantage of conspicuousness account for all the variety and beauty displayed in the males throughout the groups referred to? It must be constantly remembered that there is no place for beauty, except as it is connected with advantage of some kind, in the Darwinian hypothesis. In order to account for beauty, the hypothesis of natural selection must be supplemented by that of sexual selection; and to this cause Mr Wallace confidently appeals in order to meet the Duke of Argyll's well-urged objections to the theory. Now, the theory of sexual selection of course is no part of the theory of natural selection. It involves considerations quite different from it, and it rests upon very imperfect and scanty information. The public is looking forward with keen interest to the further elucidation of this subject in Mr Darwin's promised work, but at present sexual selection is hardly established as a *vera causa*. Assuming its existence in the animal world it presents the following difficulties and anomalies:

1. We should certainly have expected that the operation of sexual selection in producing beauty would be manifested in the sex that is sought and pursued rather than in the pursuers, but this is

quite the reverse of the fact. Not only among birds where the author's law may sufficiently account for it, but throughout the whole animal kingdom, beauty, as estimated by the human æsthetic faculty, adheres to the male sex:—to the sex which chooses and not to the sex which yields.

2. In the rude commerce of the sexes among animals as we observe it, it is probable that an excess of strength, speed, and that vigour which gives persistency in the male, would have completely dominated the superiority in grace and beauty which can only succeed by commending itself to the fastidious taste of the female—the sex of whose passions it has been bitterly written, "Nature made them blinder motions bounded in a shallower brain."

3. If all beauty, so far as it is dissociated from advantage, is due to the modification which the æsthetic appetency imposes on the sexual passion, we must attribute to whole classes and orders of animals, and especially to the females of their species, as high or even a higher æsthetic capacity than that which is possessed by the most refined and cultured of the human race. If any one doubt this, let him go from the examination of a cabinet of butterflies or a case of humming-birds to gaze in the shop-windows where the latest fashions are exhibited in the most attractive form which the shopman can devise.

4. By this explanation of the appearance and retention of beautiful colours and patterns, the difficulty is shifted but not removed. To say that beauty exists because each sex appreciates beauty in the other, is to explain a mysterious entity in the physical part of the organism by calling in a more mysterious power in the mental phenomena which characterize a species. This is quite inadmissible in an advocate of 'natural selection' in its fullest scope, because this ought to explain not only the form and structure of animals but also all their instincts, habits, and appetites. To show that birds are beautiful because they love beauty, and that they must appreciate beauty because otherwise it could not have been produced, is reasoning in a circle.

If the suggestion of some of these difficulties could induce Mr Wallace, who is so well provided for the enquiry, to study the relation of beauty, and especially of colour, to use in organisms, it is certain that new light would be thrown upon this important and recondite subject. If, after the uses of colours have been made more apparent, and the nature of beauty resolved into elements more closely allied to advantage than we have hitherto supposed, there should yet remain evidence that it has been in itself an end which a Superior Intelligence may have placed before Him as an object, Mr Wallace has proved himself candid enough to admit such an inference. This is evident from the last two chapters of this volume, treating of natural selection as applied to man. It must have cost the author something to have written those chapters; because he was perfectly aware that if they were accepted, as they probably would be by a certain class of thinkers, they would be made use of as a lever to upset the whole theory of natural selection; and if they

were rejected, their inconsistency with the preceding part of the volume would be mercilessly exposed by Darwinians. M. E. Claparède has already headed the onslaught. No one can read these chapters in connection with those that come before them without feeling that the author, as a theorist, is involved in difficulties; but the admirable clearness and originality of the views he presents will be acknowledged by all. We have no time or space to examine in detail the considerations by which he shows how when man became social and sympathetic, and the fabricator of his own clothes and tools and weapons, natural selection ceased to modify his physical structure. He concludes that man was a homogeneous race at a period when he had the form but hardly the nature of man, when he possessed neither speech nor sympathetic or moral feelings. If such a being be called man, then man had a common origin; if not, then a multiple one. To this conclusion it may be objected that unless in the primitive race there was something which necessitated a higher development,—something prophetic of a glorious future,—it is improbable that all the several races of man should have progressed in so parallel a course that they should have arrived at the like powers and possessions which we now recognize as the commonwealth of man. When, reasoning concerning other races or species, we find that they all possess a number of qualities possessed by no other races or species, evolutionists inevitably arrive at the conclusion that all these sprang from a common ancestor *who possessed all these peculiarities.* Mr Wallace arrives at precisely the opposite conclusion, namely, that the common ancestor did *not* possess what was subsequently developed in all. To give consistency to this theory he advances in his last chapter some evidence of what we have spoken of above as the prophetic peculiarities of savage man. These are the superfluously vast brain, the skin naked along the midline of the back, the structure of the hands and feet, and the modulated voice. The full uses and capabilities of these are never fully evolved in savage life. They have therefore a prophetic function. Before we agree with M. Claparède that the chapters containing these words are the product of a blind anti-Darwinian, while the others proceed from an audacious Darwinian, we must call to mind that the theory of natural selection only accounts for the conservation and not for the production of variation. Some more general and fundamental law may underlie and yet be quite consistent with it. If a number of facts point directly either to something inherent in each species which shapes itself towards a future end, or even to the controlling intelligence of a Creator consciously aiming at results, in place of the blind *quaqua*-versal variation which is tacitly assumed by most evolutionists, the deduction may nevertheless be strictly scientific.

Leçons sur la Physiologie Comparée de la Respiration, by PAUL BERT, Professor of Physiology at the Sorbonne. 8vo. pp. 588. Baillière, Paris, 1870.

Paul Bert stands facile princeps among the younger physiologists of France. Within a few years he has done an amount of work which many persons would deem sufficient for a life-time. Happily the structures which he has reared around him with such surprising swiftness have nothing of a mushroom character about them. They have been firmly built and will endure. The work before us is Professor Bert's last and greatest effort. It is certainly a somewhat ponderous tome, but its size is not due to *dilution;* it on the contrary results from the grouping together of a prodigious number of facts. These have not been hurriedly huddled together, but have been introduced and displayed with the matchless skill for which our French brethren enjoy so well-deserved a reputation.

The work is not only an agreeable and comprehensive résumé of what is already known regarding respiration, but it abounds with original observations which entitle its author to the greatest respect. The book displays much learning. In no other treatise on the respiration with which we are acquainted is the bibliography of the subject so fully given. It would have afforded us much pleasure to have furnished the English reader with a lengthened abstract of the original research contained in these lectures; but this is unnecessary, seeing that a full report of it from the pen of Dr Broadbent has already appeared in the *Brit. and For. Med. Chirurg. Rev.* for Oct. 1870. We most cordially recommend Dr Bert's book to the attention of every physiologist.

Manual of Human and Comparative Histology, edited by S. STRICKER, translated for the Sydenham Society by HENRY POWER, M.B. Lond., F.R.C.S. We are extremely glad that the contents of Professor Stricker's well-known and valuable work have been thus quickly placed within the reach of English readers by so competent a person. The remaining part will, we believe, shortly appear in the German, and Mr Power, we may be sure, will lose no time in completing the translation.

An Introduction to the Osteology of the Mammalia, by W. H. FLOWER, F.R.S., Hunterian Professor of Comparative Anatomy and Physiology, and Conservator of the Museum of the Royal College of Surgeons of England, being the substance of the Course of Lectures delivered at the College in 1870, 8vo. pp. 344, Macmillan & Co., reaches us as this sheet is passing to press. We can therefore only say that Prof. Flower's great, perhaps unrivalled, knowledge of Mammalian Osteology is a sufficient guarantee for the excellence and accuracy of the work.

REPORT ON THE PROGRESS OF ANATOMY.

By PROFESSOR TURNER[1].

OSTEOLOGY.—Wenzel Gruber, after giving an account (*Virchow's Archiv*, L. 113) of previously recorded cases of CONGENITAL SUBDIVISION OF THE PARIETAL BONE by a transverse or oblique suture, relates one in which the right parietal, in a youth, was divided by a fissure, which extended from the antero-superior angle obliquely backwards and downwards through the lambdoidal suture into the right half of the interparietal part of the occipital bone.——At p. 124 he describes a female skull in which both PARIETAL FORAMINA WERE UNUSUALLY LARGE, and a male skull with an abnormally large right parietal foramen. At p. 233 he describes a skull in which the STYLOID PROCESSES OF THE TEMPORAL BONES were each three inches long.——In LI. p. 137, Th. Simon describes a male cranium where the parietal foramina were greatly enlarged, and on p. 140, Lücke remarks on the practical bearings of abnormally long and bent styloid processes of the temporal bones.——Gruber also records in *Reichert and Dubois Reymond's Archiv*, 1870, p. 197, additional cases (*Report*, IV. 151) of PERSISTENCE OF THE STYLOID PROCESS OF THE THIRD METACARPAL AS AN EPIPHYSIS, and he figures a hand in which a ninth supernumerary carpal bone represented the persistent epiphysis of the third metacarpal.——In the same *Archiv*, p. 112, Ludwig Stieda points out that the presence of a PROCESSUS MARGINALIS on the posterior margin of the MALAR bone, which Luschka described a year ago (*Report*, IV. 150), was known to Sömmering, after whom he suggests it should be named Processus Sömmeringii.——Julius Wolff (*Virchow's Archiv*, L. 389) enquires into the INTERNAL ARCHITECTURE OF THE BONES with especial reference to the question of growth.——C. Kutschin contributes a paper (*Untersuch. aus dem Inst. in Graz*, 1870) on the DEVELOPMENT OF BONE.——M. Frankel (*Virchow's Archiv*, L. 145), gives an account of a memoir by Wyman (*Proc. Boston Soc. Nat. Hist.*, IX.) on SYNOSTOSIS OF THE CRANIUM, and records some cases observed by himself of irregularities in the condition of the cranial sutures.——J. Cleland's memoir on the VARIATIONS OF THE HUMAN SKULL, an abstract of which in *P. R. S. L.* was referred to in *Report*, IV. 151, has now appeared *in extenso* in *Phil. Trans.* 1869. The memoir is of considerable importance in connection with the methods of craniological enquiry.——W. K. Parker's memoir on the STRUCTURE AND DEVELOPMENT OF THE SKULL OF THE COMMON FOWL is also printed *in extenso* in the same Vol. of the *Phil. Trans.*——A memoir by R. Virchow on the CRANIA at Kopenhagen of the old Norse people is in *Archiv für Anthropologie*.

[1] To assist in making this Report more complete, Professor Turner will be glad to receive separate copies of original memoirs and other contributions to Anatomy.

MUSCULAR SYSTEM.—Hubert von Luschka describes and gives a drawing (*Reichert u. du Bois Reymond's Archiv*, 227, 1870) of a small muscle, which he calls M. PUBO-TRANSVERSALIS. It arises from the horizontal ramus of the os pubis, and ascends behind the fascia transversalis to end by delicate tendinous fibres in that structure, above and to the inner side of the internal abdominal ring. It is not a constant muscle.——J. Cazalis enquires into the DEVELOPMENT OF THE MUSCULAR FIBRES OF THE DIAPHRAGM (*Archives de Phys.* p. 64, 1870). He concludes that the appearance of the fibres of this muscle is not posterior to their appearance in those muscles previously known to be amongst the first to develope. Secondly, the development of these fibres at the time of birth is more advanced than that of the fibres of animal life, and points out the importance of this in connection with the regular and immediate performance of the functions of the diaphragm in the new-born child. The memoir by John Wood on VARIETIES OF THE MUSCLES OF THE NECK, SHOULDER AND CHEST, an abstract of which in *P. R. S. L.* was referred to in *Report*, IV. p. 154, is now printed *in extenso* in *Phil. Trans.* 1870.

VASCULAR SYSTEM.—Wenzel Gruber records (*Reichert u. du Bois Reymond's Archiv*, 1870) cases of IRREGULARITIES IN THE RADIAL ARTERY. In the right arm of a man the radial artery had three roots, a long upper derived from the axillary, a middle from the deep median artery, a lower from the anterior interosseous artery. In the right arm of a youth he found the radial artery rudimentary, reaching only to the middle of the fore-arm. The interosseous artery again was very large, and from its anterior branch arteries arose which in part supplied the place of the deficient radial. The ulnar also large, formed both the superficial and deep palmar arches, and supplied all the digits. In *Bull. de l'Acad. des Sc. de St Petersb.* he describes cases where the radial artery lay in the lower third of the fore-arm on its dorsal aspect before reaching the carpo-metacarpal region: also a case, where the ulnar artery having a high origin was united by a short vessel to the brachial at the bend of the elbow. ——The structure of the WALLS OF THE SMALLER BLOOD-VESSELS is now being carefully investigated in consequence of the attention which the theory of Addison, Waller, and Cohnheim, as to their perforation by the blood-corpuscles is receiving. Both in the *Monthly and Quarterly Microscopical Journals*, Oct., is a reprint of a report by J. J. Woodward on the histology of the vascular walls: he particularly describes the so-called stomata in them, which he finds to be largest and best marked in veins $\frac{1}{50}$th inch in diameter, or even larger; they are comparatively infrequent in capillaries, and still more so in the small arteries. In figure they are round, oval, or oblong: they range from $\frac{1}{2000}$th to $\frac{1}{10000}$th inch in diameter, and are almost invariably found in the marginal line between the epithelial cells. The author is inclined to think that they are actual openings in the epithelial layer. He has seen the passage of white corpuscles through the vascular walls.——R. Caton describes the method of studying transparent vascular tissues in living animals (*Quart. Mic. Journ.* July,

1870). He has examined frogs, toads, fish, tadpoles, newts, and mice. Like Stricker, Cohnheim and others, he has used a subcutaneous injection of curare, but finds that a solution of 4 grs. of chloral to a drachm of water answers the purpose of rendering the animal still under the microscope.——S. Stricker and J. Burdon Sanderson write a joint memoir on a new method of studying the capillary circulation in mammals (*Quart. Mic. Journ.* Oct. 1870): they chloralize a guinea-pig, open the abdominal cavity, and examine the omentum laid out in a glass dish, and floated in a warm bath which contains common salt in solution.——Victor von Ebner describes (*Untersuch. aus dem Phys. Inst. in Graz*, 1870) the STRUCTURE OF THE WALL OF THE AORTA, more especially its muscular coat.

NERVOUS SYSTEM.—Several papers on the PERIPHERAL TERMINATION OF NERVES have recently appeared. W. Krause investigates the *ending of nerves in smooth muscles*, and selects the *m. recto-coccygeus* in the rabbit (*Reichert u. du Bois Reymond's Archiv*, April, 1870). He describes a medullated nerve-fibre becoming continuous with one or several pale nerve-fibres, which last have mostly a dichotomous subdivision. These pale fibres end in connexion with three or four oval nuclei with homogeneous pellucid contents—the special end-organ—which can be distinguished from the elongated nuclei of the muscular fibre and from the nuclei of the capillaries. In the striped muscles each fibre has its own special end-organ, in the non-striped hundreds of muscle-spindles are dependent on one terminal nerve-organ.——A. Hénocque (*Archives de Phys.*, May, 1870) also inquires into the same subject, and selects especially the muscular wall of the bladder for observation. He finds a nervous plexus situated in the connective tissue which surrounds and separates the muscular fasciculi, and a more delicate plexus in the interior even of the fasciculus. The terminal fibrils from this latter plexus subdivide dichotomously where they anastomose, and end in a minute knob or swelling, which is situated frequently near to the nucleus of the smooth fibre, or at the surface of the fibre, or between adjacent fibres.——W. Krause also examines the *endings of nerves in glands* (*R. u. du Bois Reymond's Archiv*, April, 1870), especially the salivary. His observations are made with especial reference to the determination of their connection with the secreting cells of the glands, which Pflüger and others have described. He has not been able to satisfy himself that the so-called secretory end-bodies exist within the acini, and has not therefore been able to confirm Pflüger's observations. In the same *Archiv*, p. 238, Ihlder of Göttingen describes the termination of the *nerves in the tongues of birds*. In the papillæ terminal bodies, which he names 'Tast-Kolben,' exist, ellipsoidal or cylindrical in form, with rounded ends; composed externally of a sheath of connective tissue, on the inner wall of which nuclei are arranged transversely. In the axis of each body a pale nerve-fibre lies which ends in a well-defined swelling.——Hans v. Wyss investigates the *goblet-shaped organs of the tongue* (*Schultze's Archiv*, 1870, p. 237). He examines these structures in man and several other mammals. In

man he recognises them in great numbers on the lateral surfaces of the papillæ circumvallatæ, where they are arranged in 5 to 6 superimposed rows. They consist of two elements, investing or covering cells, and bacillary or sensory cells. The covering cells consist of several layers and are so arranged that they enclose a space of a goblet shape, their peripheral ends permit and surround a small aperture, whilst their central ends sink into the fibrous substance of the papilla. The bacillary cells occupy the space enclosed by the covering cells: they are elongated and somewhat spindle-shaped, and vary in number with the size of the goblet. The peripheral end is frequently very elongated, and may even project through the aperture surrounded by the peripheral ends of the covering cells: the middle portion of the spindle contains a large nucleus surrounded by a little protoplasm: the central end of the spindle is often irregularly bent, and it is possible may be connected with the minute terminal nerve-fibres. Wyss has not seen these goblet-shaped bodies on the papillæ fungiformes of the human tongue. He then describes their arrangement in the ox, sheep, pig, horse, hedgehog, rabbit, rat, and squirrel, and concludes that they have a special relation to the nerves of taste, and are peripheral end-organs in connection with these nerves.——F. E. Schulze describes, p. 407, *taste-organs* of a somewhat similar character in connection with the tongue of a larval amphibian, *Pelobates fuscus*.

EYE-BALL.—W. Krause contributes some observations on the ANTERIOR EPITHELIUM OF THE CORNEA (*Reichert u. du Bois Reymond's Archiv*, July, 1870). He refers especially to the deepest layer of cylindrical epithelium on the anterior surface of the cornea, which contain very granular bodies, which when isolated possess no trace of an investing membrane.——B. Wende gives in April number of the same *Archiv* a short paper on the CILIARY MUSCLE.——J. W. Hulke describes (*Quart. Mic. Journ.*, October, 1870) the minute anatomy of those parts of the eye-ball which are connected with ACCOMMODATION.——G. Schwalbe has now completed his account of the LYMPH-SPACES IN THE EYEBALL (*Schultze's Archiv*, 1870, pp. 1. 261).

GLANDS —R. Heidenhain investigates the structure of the PEPTIC GLANDS in the gastric mucous membrane (*Schultze's Archiv*, 1870, 368). In addition to the cylindrical epithelium at the entrance to the glands, he distinguishes within the gland two kinds of cells, one not coloured by carmine, very numerous in the body of the gland, and also seen in its neck, which he calls the principal cells (*Haupt-zellen*), the other coloured by carmine and almost always placed outside the first-named, which he calls *Beleg-zellen*. Although variations may take place in the number of *Beleg-zellen*, yet the general principle of the structure is everywhere the same, an inner formation of *Haupt-zellen* and an outer formation of *Beleg-zellen* is repeated everywhere. Heidenhain makes no reference to the observations of the late Dr Brinton (Article *Stomach* in *Cyclop. of Anatomy*, also in *Med. Chir. Rev.*, July, 1862), in which that observer points out, that the dimor-

phous structure of the secreting glands of the stomach occurs not only in the dog, as Kölliker indeed first described, but is a character which extends without exception throughout the vertebrate class.

In a memoir on the STRUCTURE OF THE HUMAN LIVER (*Month. Micros. Journ.*, Aug. 1870), H. D. Schmidt states that the hepatic veins with their nerves and lymphatics possess a capsule of connective tissue, which differs only from Glisson's capsule in being less strongly developed; this capsule receives small branches from the hepatic artery. He describes the finest branches of the lymphatic vessels in the liver as arising, like those of the hepatic duct, from a network of "biliary tubules" by which term he means a capillary network, independent of the blood-capillaries, which exists in the parenchyma of the liver. Although he does not refer to previous observations, it is evident that his "biliary tubules" are the minute bile-passages which Hering, Eberth, and Kölliker have also injected (see Vol. II. of this *Journal*). He believes that the passage of an injection from the hepatic duct through the biliary tubules into the lymphatics, which he has several times observed, is not the result of extravasation, but of a true continuity of one set of tubes with the other.——Silver Amado in a note on an obscure point in the HISTOLOGY OF THE THYROID GLAND (*Robin's Journal*, 1870, 244), describes various prismatic crystals which he has met with in this gland; which crystals are isomorphous with creatine. The Reporter may take this opportunity of stating that in 1860 (*Trans. Roy. Soc. Edinb.*) he found a number of three-sided prismatic crystals, soluble in acetic acid without effervescence, lying in the connective tissue between the lobules of the thymus gland of the Nylghau (*Antilope picta*).

MALFORMATIONS.—Reference is made in *Virchow's Archiv*, L. 296, 297, to a case of TRIPLICITAS MONSTROSA INFERIOR, and one of DUPLICITAS MONSTROSA SUPERIOR; and on p. 303, a monstrosity in the Goat: ACARDIACUS ACEPHALUS BIPES.——In Vol. IV. of this *Journal*, pp. 89, 200, John Chiene and John Cleland have described cases of a SUPERNUMERARY LOBE TO THE RIGHT LUNG. Wenzel Gruber has also recorded two similar cases (*Bull. de l'Acad. Imp. des Sc. de St. Petersb.*, 5 April, 1870). In both the additional lobe was derived from the upper lobe of the lung, by the presence of a vertical fissure in which the vena azygos was placed.——In *Virchow's Archiv*, L. 633, Ponfick describes a case of congenital atrophy of the right lung, where the right bronchus was developed, but the right half of the thorax was filled with a reddish-yellow gelatinous tissue. In the midst of this tissue was an ovoid body, 5 lines long, 3½ broad, and 2½ thick, with which the right bronchus was connected.

EMBRYOLOGY.—M. Balbiani communicates (*Ann. des Sc. Nat.*, XIV.) the first part of a memoir on the GENERATION OF APHIDES. ——C. Ritsema describes the ORIGIN AND DEVELOPMENT OF PERIPHYLLUS TESTUDO: translated in *Ann. of Nat. Hist.*, July, 1870.——

A. Golubew contributes a paper (*Untersuch. aus dem Phys. Inst. in Graz*, 1870) on the DEVELOPMENT OF THE BATRACHIA.——Anton Dohrn describes his researches (*Siebold u. Kolliker's Zeitsch.*, 1870) on the STRUCTURE AND DEVELOPMENT OF THE ARTHROPODA.——A. Schneider investigates the DEVELOPMENT OF AURELIA AURITA (*Schultze's Archiv*, 1870, 237).

COMPARATIVE ANATOMY AND MORPHOLOGY.—J. E. Gray criticizes (*Ann. Nat. Hist.* Sept. 1870) the descriptions of the GENUS BALÆNA given by MM. Beneden and Gervais in the *Ostéographie des Cétacés.* ——In the October number of the *Annals* the same author figures and gives a brief description of the skeleton of DIOPLODON SECHELLENSIS.——W. H. Flower (*Proc. Zool. Soc.* Dec. 9, 1869) describes a specimen of BALÆNOPTERA MUSCULUS beached at Langston harbour in November 1869, and gives notes on the skeletons of the same species of whale stranded at Margate, Falmouth (the skeleton of which is in the Alexandra Park), and Black Gang Chine. The main differences between these three skeletons, besides size, are in the extent of the development of the lower transverse process of the 6th cervical vertebra, in the presence or absence of a capitular process to the 1st rib, and in the development of the 15th rib. These prove how large an amount of variation, quite independently of age, may exist in different individual cetacea which may in all probability be assigned to the same species.——P. J. van Beneden in an interesting lecture (*Bulletins de l'Acad. Roy. de Belgique*, No. 12, 1869), entitled COMMENSALISM IN THE ANIMAL KINGDOM, discusses the association with each other of certain animals. He distinguishes between *parasitism* and *commensalism*. A *parasite* is that which feeds on another, an *associate* (*commensal*) is simply a table-companion which attaches itself to another and larger animal in order to obtain food. Commensals may be either free or fixed, the former are the most numerous. The cirripeds attached to the cetacea are good examples of the fixed. ——In the same *Bulletins*, No. 4, 1870, he catalogues the COMMENSALS AND PARASITES OF THE CETACEA. *Balæna mysticetus* with Cyamus ceti and Echinorhynchus mysticeti. *B. biscayensis* with Cyamus biscayensis and Coronula biscayensis. *B. australis* with Coronula balænaris, Tubicinella trachealis, Cyamus erraticus, Odontobius ceti, Pirolina ceti, Acarus balænarum. *B. japonica* with Diadema japonica. *Megaptera boops* with Diadema balænaris, Conchoderma auritum. *M. lalandii* with the same. *M. Novæ Zelandiæ* with Diadema . . . . *M. antarctica* with Diadema californica. *Balænoptera rostrata* with Echinorhynchus porrigens, Distoma goliath, Filaria crassicauda. *B. musculus* with Penella balænopteræ, Echinorhynchus . . ., Monostomum plicatum. *B. sibbaldii* with Echinorhynchus brevicollis. *Physeter macrocephalus* with Conchoderma cuiverii, oniscus, and cysticercus. *Hyperoodon butzkopf* with Penella crassicornis, Conchoderma cuvierii, Cyamus thompsoni, Echinorhynchus turbinella, Monostomum delphini. *Micropteron sowerbiensis*

with Monostomum delphini. *Dioplodon europæus* with Conchoderma cuvierii. *Platanista gangetica* with Ascaris delphini. *Delphinus delphis* with Lerneonema nodicornis, Echinorhynchus pellucidus, Phyllobothrium delphini. *D. amazonicus* with Peritrachelius insignis. *D. rostratus* with Tetrabothrium triangulare. *D. tacuschi* with Distomum lancea. *Lagenorhynchus eschrichtii* with monostoma. *Phocæna communis* with Ascaris simplex, Strongylus inflexus, S. minor, S. convolutus, Filaria inflexicaudata. *P. compressicaudata* with Cysticercus. . . . *Globiceps melas* with Cirolana globicipitis, Xenobalanus globicipitis, Conchoderma cuvierii, Cyamus globicipitis. *Beluga leucas* with Strongylus pallasii. *Monodon monoceros* with Cyamus monodontis and nodosus and with Strongylus alatus.

W. H. Flower describes in *Proc. Zool. Soc.* Nov. 11, 1869, the ANATOMY OF PROTELES CRISTATUS. Though still inclined to place *Proteles* in a family by itself, allied to both Hyænidæ and Viverridæ, the examination of this specimen showed that its affinities with the former family are closer than the examination of the skull alone had previously led him to suppose.——G. Gulliver gives an account (*Proc. Zool. Soc.* Feb. 10, 1870) of the RED BLOOD-CORPUSCLES in *moschus, tragulus, orycteropus, ailurus. Tragulus* has the smallest blood-corpuscles amongst mammals. In *T. javanica* and *T. meminna* the average diameter is $\frac{1}{12325}$ of inch and the extreme sizes $\frac{1}{16000}$ and $\frac{1}{9600}$ of inch: in *T. stanleyanus* the average is $\frac{1}{10825}$ of inch. In *Moschus moschiferus* the average is $\frac{1}{7000}$ extremes from $\frac{1}{8888}$ to $\frac{1}{5333}$. In *Orycteropus capensis* average $\frac{1}{2769}$, the extremes being one-third smaller and one-third larger than average. These corpuscles are amongst the largest known in the mammalia. In *Ailurus fulgens* the average is $\frac{1}{3764}$ of inch. These observations the author considers support his view, that amongst the mammalia the smallest blood-disks occur in the small species of an order or family, the largest blood-disks in the large species of that order or family.——Jas. Murie describes (*Proc. Zool. Soc.* Jan. 27, 1870) a LARVAL ŒSTRUS FOUND IN THE HIPPOPOTAMUS, and on Jan. 13, 1870, he communicates additional memoranda as to IRREGULARITY IN THE GROWTH OF SALMON. He considers that he has collected evidence which substantiates the view that the *Salmo salar* can live for a series of years in fresh water without access to the sea, and that a very appreciable arrest of growth is the consequence where retention to a limited area of water obtains.——E. Ray Lankester and St George Mivart have been discussing (*Annals Nat. Hist.* July, August, Oct. 1870) the use of the term HOMOLOGY. Lankester proposes to abandon the old term Homology and to substitute for it two new terms, Homogeny and Homoplasy. By homogeny he means structures which are genetically related in so far as they have a single representative in a common ancestor; and as examples of this relation he gives, so far as their most general structure is concerned, the fore-limbs of mammalia, sauropsida, batrachia and fishes. By homoplasy he includes all cases of close resemblance of form which are not traceable to homogeny, all details of agreement not homo-

geneous, in structures which are broadly homogeneous, as well as in structures having no genetic affinity. Mivart, whilst acknowledging the usefulness of the terms proposed by Lankester, yet considers that it is not possible always to discriminate between homogeny and homoplasy, and that the term homology ought still to be retained and used in the sense given to it by Owen, viz. to express a close resemblance of parts as regards their relation to surrounding parts, to whatever cause that resemblance may be due, whether genetic or otherwise. He suggests the term Actinology to denote that kind of homological relation which exists between the successive segments, regions, or divisions of a part or organ.—— Carl Gegenbaur (*Jenaische Zeitsch.* v. 397) continues his researches into the constitution of the LIMBS IN THE VERTEBRATA, and of the HIND-LIMBS OF THE SELACHIA. The first section is devoted to the hind-limbs of the Selachia, the different modifications of the skeleton in sharks and rays, and the parts in the different genera which correspond with each other. In the 2nd section the fore- and hind-limbs are compared with each other, and an attempt is made to show what is the fundamental basis-form of each extremity. In the 3rd section he describes the amount of variation met with in the skeleton of the limbs in the Selachia. In the 4th section he proceeds to show how the skeletons of the limbs of the other vertebrates are reducible from the archetype which he has arrived at from the consideration of the Selachian limbs.

I. Archipterygium with an inconstant number of radii.

(Polyactinic form.)

1. Shaft and radii dissimilarly differentiated.

a.	*b.*	
unaltered,	altered,	
Dipnoi,	through fusion of radii, hind-limbs of *Chimæra,*	and separated through detachment and through connexion of radii with the limb-girdle. *Chimæra* (fore-limbs), *Selachia.* With peripherical reduction. *Ganoids, Teleosteans.*

2. Shaft and radii similarly differentiated.

Ichthyosaurus.

II. Archipterygium with a constant number of radii.

(Tetractinic form.)

Transverse differentiation into individual constant segments.

1. Hexameric fundamental form of Carpus and Tarsus. *Plesiosaurus.*	2. Decameric fundamental form of Carpus and Tarsus. *Amphibia, Reptiles, Birds, Mammals.*

The last section is devoted to the consideration of the clappers in the male Selachia and Chimæra, and reasons are given for regarding them as homologues of the hind-limbs of other vertebrates.——Gustav. Fritsch communicates (*Reichert u. du Bois Reymond's Archiv*, Feb. 1870) a long memoir with many illustrations on the HEART IN THE AMPHIBIA. He describes the external form and position of the heart and its subdivisions, the arrangement of the great vessels, their development from the vascular arches of the embryo, the internal structure of the heart, and the function of the various parts. ——A. Kölliker shows (*Verhand. der phys. med. gesellsch. zu Würzburg*, Feb. 5, 1870) that SEXUAL REPRODUCTION occurs in some species of Virgularia and Pennatula, not in the zooids which are provided with tentacula, and which otherwise seem to be mature, but in zooids not provided with tentacles, and situated lower down on the parent stem. The sexual products in the lowest polypes are small and feebly developed, higher up they are more mature. Three kinds of polypes are therefore seen on the parent stem: 1st, feebly developed with tentacles: 2nd, undeveloped without tentacles, with sexual organs: 3rd, rudimentary without tentacles or sexual apparatus. The sexual and nutrient polyps are therefore young and old. Part of the sexual are converted into nutrient polyps, all the nutritive were once sexual and undeveloped polyps: sexual reproduction did not commence only when the parent stem was fully developed. On the 26th Feb. Kölliker described a new Alcyonarian polyp, *Pseudo-gorgia Godeffroyi*, which forms a connecting link between the Gorgonidæ, Alcyonidæ and Pennatulidæ. The specimen came from the gulf of St Vincent, Australia.——G. Pouchet and Myevre contribute (*Robin's Journal*, 1870, p. 285) a memoir on the ANATOMY OF ALCYONARIA.——R. Hartmann contributes to the ANATOMY OF PARASITIC CRUSTACEA (*Reichert u. du Bois Reymond's Archiv*, April, 1870). The species which he describes is the Bomolochus belones.

REPORT ON THE PROGRESS OF PHYSIOLOGY, from 1st April to 1st September, 1870. By THOMAS R. FRASER, M.D., F.R.S.E., F.R.C.P.E., *Assistant Physician to the Royal Infirmary, Edinburgh;* ARTHUR GAMGEE, M.D., F.R.S.E., *Lecturer on Physiology at Surgeons' Hall, Edinburgh;* and T. LAUDER BRUNTON, M.D., D.Sc., *Lecturer on Materia Medica at the Middlesex Hospital, London.*

DR FRASER'S REPORT.

Physiological Action of Medicinal and Poisonous Substances.

BROMIDE OF SODIUM.—Following a suggestion advanced by M. Balard to the French Academy of Sciences, M. E. Decaisne has made some experiments and observations with bromide of sodium as a substitute for the corresponding salt of potassium (*Comptus Rendus*, 25 April, 1870, p. 947). He finds that it is a perfect substitute for bromide of potassium in the treatment of epilepsy, chorea, &c., over which it has the advantage of being more readily eliminated, and, therefore, more capable of being administered in large doses for a long period. A difference is pointed out in the action on the digestive system; bromide of sodium frequently causing thirst and constipation, whereas bromide of potassium is apt to produce colic and diarrhœa. Decaisne believes that the action of the bromides depends on bromine.

SULPHATE OF MAGNESIA.—In order to determine the manner in which the purgative effects of sulphate of magnesia are produced, M. Armand Moreau (*Archives Générales de Médecine*, Août, 1870, p. 234) undertook a series of experiments, of which the most important consisted in placing some of this salt in contact with an exposed knuckle of intestine in a living animal. He observed that this nearly immediately produced an increased flow of liquid; and to this flow M. Moreau attributes the purgative effects.—An explanation contrary to this has been advanced by Dr L. Radziejewski, who ascribes the action of purgatives to increased intestinal peristalsis (Zur physiologischen Wirkung der Abführmittel, *Reichert und du Bois Reymond's Archiv*, 1870, No. 1, p. 37).

ALCOHOL.—We apprehend that, among the many recent investigations into the effects of alcohol, that of Professor Parkes and Count Cyprian Wollowicz (*Proceedings of the Royal Society*, No. 120, 1870, p. 362) is destined to occupy a prominent position in the discussion of the effects of this agent on the healthy human body. The researches were made on a soldier, twenty-eight years of age, selected mainly on account of his remarkably good physical condition, and superior intelligence. During a preliminary period of ten days he abstained altogether from any alcoholic liquid; and as he was accustomed to smoke, it was considered proper to allow him half an ounce of tobacco daily, during the whole period of the observations. The plan pursued was as follows: "For twenty-six days the man remained on a diet precisely similar as to food and times of meals in every respect, except that for the first eight days he took only water

(in the shape of coffee, tea, and simple water); for the next six days he added to this diet rectified spirit in such proportion, that he took, in divided quantities, on the first day one fluid ounce (= 28·4 cub. centims) of absolute alcohol; on the second day, two fluid ounces; on the third day, four ounces; and on the fifth and sixth days, eight ounces on each day. He then returned to water for six days; and then for three days took on each day half a bottle (= 12 ounces, or 341 cub. centimes) of fine brandy, containing 48 per cent. of alcohol. Then for three days more he returned to water." The observations thus include five stages; of water-drinking, of alcohol, of water, of brandy, and finally of water. The quantities of alcohol given were never sufficient to produce any extreme symptoms of narcotism, as the object of the investigation was to examine the *dietetic effects* only. The results obtained were on the whole of a negative character, so far at least as any marked modification of the vital processes is concerned. Firstly, it was found that, other conditions remaining constant, alcohol had no important influence in modifying *weight*. Secondly, one or two fluid ounces of absolute alcohol given in divided quantities in twenty-four hours served to increase the *appetite;* but four fluid ounces lessened it considerably, and larger quantities almost entirely destroyed it. Thirdly, neither pure alcohol nor brandy, in the quantities given, lessened the *temperature*. Fourthly, judging by the evidence supplied by the characters of the pulse, the state of the cutaneous vessels, and the nature of the sphygmographic tracings, the chief action of alcohol on the *circulation* in health is exerted on the ventricles of the heart and on the capillaries. In the former, the rapidity with which the contractions are accomplished is greatly increased; and in the latter, the calibre is increased so that the blood flows more freely through them. Fifthly, the amount of *urinary water* is somewhat increased. Sixthly, alcohol, in the quantities mentioned, produces no effect of importance in altering the *elimination of nitrogen by the kidneys* in the healthy body, when the ingress of nitrogen is constant. This conclusion is one of considerable importance, as previous observers have found that nitrogen is largely retained in the body when alcohol is given, thereby increasing assimilation, or, when food is deficient, saving the tissues from destruction. Seventhly, the influence of alcohol on the *phosphoric acid, chlorine, and free acids of the urine* is inconsiderable. Eighthly, the *nervous system* was not shown to be affected by any evidence of increased diminution in the amount of phosphoric acid, but there were marked subjective phenomena. Ninthly, the *elimination of nitrogen by the bowels* was not lessened. Tenthly, the *elimination of alcohol* from the body would appear to be a slower process than has generally been supposed. Thus, after twenty-nine fluid ounces of absolute alcohol had been taken in six days, the body had still traces of it on the sixth day after the alcohol was left off. The experiments, however, do not shew whether all the alcohol is eliminated, or whether some is destroyed in the body.

Acetic Ether.—Dr H. C. Wood suggests that acetic ether should be employed as an anæsthetic (*American Journal of the Medi-*

cal Sciences, July, 1870, p. 137). He bases his suggestion on the facility with which it produces anæsthesia in the lower animals, and this with much less previous struggling than with sulphuric ether. Over the latter it has also the advantage of a higher boiling point, and a lower degree of volatility.

CHLOROFORM.—The effects of chloroform on the irritability of the sensitive stamens of Mahonia have been examined by M. Jourdain (*Comptus Rendus*, 25 April, 1870, p. 948). One minute after the exposure to the vapour of chloroform, these stamens became strongly flexed backwards, as if in tetanus, and insensible to all excitation. The irritability returned after free access of air; imperfectly in eight or ten minutes, but perfectly in from twenty-five to thirty minutes. When the exposure to chloroform extended over two or three minutes, the same effects were produced, only the return to the normal sensitive condition was slower. An exposure of from ten to fifteen minutes resulted in the death of the whole flower.

BROMAL-HYDRATE.—Dr. E. Steinauer has been led to conclude from a series of experiments on animals, that hydrate of bromal is in the system converted, in the first place, into bromoform, and then into bromides (*Virchow's Archiv*, July, 1870). He found that it is an active toxic substance, producing in small doses (a grain and a half for dogs) death, preceded by hyperæmia of the nasal and oral mucous membranes, anæsthesia, and convulsions. (Our own experiments, lately performed, more or less coincide with those of Dr Steinauer, excepting that the hyperæmic effects were even more marked. We invariably found that the smallest dose which could produce any impairment of sensibility in rabbits, likewise produced great dyspnœa and death; while the autopsy revealed an extreme degree of congestion of the lungs and respiratory passages. These effects may probably be explained by an impurity of the preparation, as it has recently been found that the more frequently hydrate of bromal is purified by recrystallization, the less apt is it to excite irritation of the respiratory organs).

SULPHOVINATES, their elimination and action.—Dr Rabuteau has gained a deserved reputation by his important investigations into the action of numerous inorganic substances. His recent examination of the sulphovinates in every way maintains his credit (*Gazette Hebdomadaire*, 10 June, 1870, p. 356). The sulphovinates are somewhat unstable substances, as they gradually decompose in water, reproducing alcohol and setting free sulphates. Dr Rabuteau made some experiments to determine if this decomposition occurs in the living organism. He administered salts of this class to various animals by injection into the veins and introduction into the stomach, and found that they were eliminated by the urine partially unchanged and partially in the form of sulphates. His experiments prove that sulphovinate of sodium is an inoffensive salt, and that when injected into the blood it is rapidly eliminated, producing, however, very decided constipation. This effect suggested that administration by the stomach would result in the production of catharsis, in conformity with the behaviour of purgative salts in general. A large number

of experiments were made on man, in a state of health and of disease, which confirmed his supposition, and demonstrated that in doses of from about one-quarter to three-quarters of an ounce sulphovinate of sodium is an efficient, mild, and rapid purgative, whose action is not accompanied with griping, nor followed by constipation.

QUINIA.—The existence of a centre in the brain that more or less controls the temperature of the body has been proved by many recent observations. It may also be assumed, as a result from experimental investigation, that at least in certain conditions of the body several active substances, and notably quinia and alcohol, have the power of reducing abnormal elevations of temperature. By many pharmacologists it is maintained that this action of quinia and alcohol is exerted through an influence on the moderating centre. In a recent paper, Professor Binz (*Practitioner*, July, 1870, p. 1) opposes this view, and maintains that the antipyretic action of quinia and alcohol is due to their influence in diminishing oxidation. In support of his assertion, an ingenious experiment is described, in which the influence of the moderating centre was removed in a dog by division of the spinal cord between the sixth and seventh cervical vertebræ, and then a large dose of quinia exhibited. The previous separation of the moderating centre of warmth from the whole body did not at all prevent the antipyretic action of quinia. By a similar method of research, it was shewn that the antipyretic action of alcohol is independent of an irritation of the moderating centre[1].

GELSEMIUM SEMPERVIRENS.—Professor Bartholow has arrived at the following results from an examination of the physiological action of Gelsemium sempervirens (*Practitioner*, October, 1870, p. 200). 1. It has a selective action on the nervous system. 2. It acts chiefly on the motor portion of the cord. 3. Its paralysing action is due to an influence on the motor centre, and not on the peripheral nerve-fibres. 4. It produces complete anæsthesia in cold-blooded animals by acting upon the sensory portion of the cord; but in warm-blooded animals this effect is toxic only, and follows the paralysis (partial?) of the motor functions.

CHLORIDE OF OXETHYL-STRYCHNIUM.—In 1869, M. Strecker discovered a new derivative of strychnia composed of strychnia + $(C^2H^5O)Cl$, to which he gave the name chloride of oxethyl-strychnine. At the suggestion of Professor Vierordt, M. E. Vaillant examined the action

[1] It will be observed that Dr Binz assumes it to be perfectly established that alcohol reduces the temperature of the body in pyrexia. This assumption is not in contradiction to the results mentioned above as obtained by Professor Parkes and Count Wollowicz. The observations of the latter had reference to the action on the healthy body only. A condition of pyrexia is dependent on increased oxidation of the tissues, and the researches of Binz go far to prove, if indeed they do not altogether do so, that the antipyretic action of alcohol is produced by the diminution of this increased oxidation. It is difficult to explain why the opposing action of alcohol to oxidation is not sensibly manifested when moderate doses are given in health. Still, the evidence is most convincing that in pyrexia alcohol usually diminishes temperature. The production of this well-established effect legitimately demands an explanation, and this explanation must in the meantime be searched for apart from such distracting results of researches into the action of alcohol in health as have been obtained by Parkes and Wollowicz.

of this substance, chiefly to determine if it produces effects similar to those of the methyl and ethyl derivatives of strychnia. His experiments appear to shew that it does so (*Journal de l'Anatomie et de la Physiologie*, No. 3, 1870, p. 256), the most prominent symptom observed being paralysis, and this being dependent on a curara-like action. In addition, however, it was observed that a certain, though comparatively slight, degree of strychnia action was produced. (Regarding the latter, or spasmodic action, it may be almost positively asserted that its production was due to some unmodified strychnia. M. Vaillant mentions that his preparation was obtained from Professor Strecker's laboratory, and was of absolute purity (une pureté absolue). It is, however, necessary to remember, as has been already pointed out by Professor Crum Brown and myself, that a distinction must be drawn between *chemical* and *physiological* purity. What would be regarded as absolutely pure by a chemist, and would be so as far as chemical testing is concerned, may be found to be impure by the physiologist, and may be unfailingly demonstrated to be so by physiological testing.)

Hyosciamia and Daturia.—An important investigation has been published by MM. Oulmont and Laurent on the action of hyosciamia and daturia (*Archives de Physiologie*, Nos. 2 and 3, 1870). These alkaloids are shewn by their experiments to possess an action very similar to that of atropia, as previous researches had already demonstrated. The conclusions arrived at are: 1. Hyosciamia and daturia act especially on the sympathetic nerve-system. 2. In small doses they diminish the capillary circulation, and in large doses they cause a vascular paralysis. 3. The arterial tension is augmented by feeble doses, while it is diminished by poisonous doses. These effects are not modified by previous division of the pneumogastrics. 4. The number of the pulsations is increased and their amplitude diminished. 5. Cardiac action is rendered regular by hyosciamia; but daturia often causes intermissions of the heart's contractions. When directly applied to the surface of the heart, both alkaloids diminish the frequency of the contractions, and then completely arrest them. 6. They always accelerate respiration. 7. Neither alkaloid exerts any primary action on the cerebro-spinal nervous system; still with poisonous doses the peripheral sensibility is blunted. 8. They do not influence the excitability of striped-muscles, nor modify their structure. 9. In feeble doses they accelerate the intestinal movements, and in large doses paralyse them. 10. The general symptoms may be referred to modifications of the circulation. They soon disappear, and the alkaloids themselves are rapidly eliminated from the system, chiefly with the urine. 11. The dilatation of the pupil which they produce is due to an excitation of the grand sympathetic; the third nerve is unconcerned in the production of mydriasis. 12. Small doses usually cause a slight augmentation of temperature, while large doses diminish the internal temperature.

Digitalis.—M. Lorain's work on the pulse (*Études de Médecine Clinique, &c.* (*Le Pouls*), 1870, pp. 327—356) contains an elaborate study of the action of digitalis, from which we content ourselves with

abstracting the following short résumé of the effects of this remedy: Digitalis augments the force of the heart, and diminishes and regulates the beats when given in moderate doses. Too large doses disturb the cardiac action.—Some carefully-devised experiments have been made by M. Mégevand to determine the action of digitalis upon nutrition (*Gazette Hebdomadaire*, 12 Août, 1870, p. 500); and for this purpose its influence on the excretion of urea was chiefly examined. It was found that digitalin, and especially digitalis, diminish the urea to a marked extent; and that this diminution seems intimately related to a simultaneous slowing of the circulation, which was always caused. Another decided effect observed was an increase in the quantity of the urine.

VERATRUM VIRIDE.—Dr Squarry (*Practitioner*, Vol. IV. 1870, p. 211) has undertaken a number of observations with the tincture of veratrum viride in order to test Oulmont's assertion, that this drug reduces the temperature of the body (see abstract of Oulmont's paper, *Journal of Anatomy and Physiology*, Vol. II. 1868, p. 424). He failed in confirming this assertion, although sufficiently large doses were given to reduce the pulse most markedly.

ERGOT.—It has been frequently shewn by experimental research that ergot contracts the minute arteries, and so increases blood-pressure. These points are elaborately confirmed in an investigation by Dr. Ch. L. Holmes (*Archives de Physiologie*, No. 3, 1870, p. 384), who examined the effects on the circulation of the aqueous extract, or egotin of Bonjean; in the first place, by observations on the vessels of the web and tongue of frogs, and subsequently by experiments on warm-blooded animals with the hæmadynamometer. From the data thus obtained it is concluded: 1st, that ergot contracts the minute vessels by an action on their muscular walls; 2nd, that this contraction augments the blood-pressure in the large vessels; 3rd, that previous division of the vaso-motor nerves does not prevent the diminution in the calibre of the minute vessels; and 4th, that this effect is also produced in the vessels of the lungs, whereby a temporary diminution in the pressure of the sytemic arteries is produced. This last result is an interesting one. It at first appeared unexplainable and contrary to expectation, and M. Holmes has devoted a considerable portion of his paper to its elucidation. He found that when a solution containing fifteen grains of the extract was injected into the jugular vein of a dog, the blood-pressure almost immediately *fell with great rapidity*, continued depressed for several seconds, and then rose until it reached a level higher than it possessed before the injection. This rapid primary descent was the remarkable and unexpected phenomenon. At first sight, it seemed explainable by the sudden contact of a foreign substance within the interior of the heart. Many ingenious experiments were made to confirm or disprove this supposition. Other substances, in themselves innocuous, as water and solution of carbonate of soda, were substituted for the solution of ergot, but their injection was not followed by sudden diminution of pressure. The injection of the ergot solution was then made into a vessel at a distance from the heart, and experiments were also per-

formed in which the cardiac nerves were divided previously to the exhibition of ergot; but the descent of the pressure-curve still occurred. It was then suggested that a contraction of the pulmonary vessels might explain the phenomenon, such contraction impeding or even preventing the passage of blood into the left heart, and so diminishing the flow into the arteries and lowering their tension. M. Holmes adopts this as the true explanation, and cites in its support the production of the same rapid diminution of pressure by the injection of powder of lycopodium, and by ligature of the portal vein and the inferior *vena cava.*

VACCINE-VIRUS.—The analogies between vaccine-virus and ordinary ferments are occupying the attention of M. Melsens, who has lately succeeded in proving that the former substance resembles alcoholic ferment in being able to withstand the influence of an extremely low temperature (*Comptus Rendus*, 4 Julliet, 1870, p. 73). Four sealed capillary tubes containing the virus were inclosed in a tube of thin glass, and this was inserted into a large tube filled with solid carbonic acid. By adding to the latter, from time to time, fresh quantities of solid carbonic acid and refrigerated ether, a temperature of -78° cent. was maintained for an hour and a half. The properties of the virus were then examined by inoculations in children, by Dr Jacobs of Brussels, and found to be unchanged. M. Melsens proposes to ascertain whether vaccine-virus resembles alcohol ferment in susceptibility of self-reproduction outside the body; and we look forward with interest for the results of these experiments.

ACTION OF VARIOUS PRINCIPLES DERIVED FROM BILE.—MM. Feltz and Ritter conclude from a large series of experiments (*Journal de l'Anatomie et de Physiologie*, No. 3, 1870, p. 315) that the salts of the biliary acids have an undoubtedly poisonous action, due to their power of dissolving the blood-corpuscles; and that a similar action, though of less activity, is possessed by taurine, glycocol; choloidic acid, and dislysine. Very slight physiological effects were produced by bilirubine, and biliprasine. Cholesterine appeared to be physiologically inert, but still it was capable of causing disturbance to the system by being precipitated in the blood, and so producing embolisms.

THE ACTION OF ALKALIES ON THE BODY.—Many therapeutists, adopting the teaching of Mialhe, maintain that alkalies are powerful oxidizing agents, which augment the excretion of urea and carbonic acid, and excite the circulation. If their action be such, they ought to prove valuable remedies in glycosuria and albuminaria, and indeed act as general tonics by improving the nutrition of the body. Experience has, however, shewn that this is not the case; but until recently we have had but a vague notion of the changes produced in the body by alkaline remedies. Some observations recently made by MM. Rabuteau and Constant (*Comptus Rendus*, 18 Julliet, 1870, p. 231) supply us with definite facts regarding the action of these remedies, and harmonize in a remarkable manner the teachings of experience with those of scientific research. From careful observations made on themselves and on patients, these experimenters have found that bicarbonate of potash or soda invariably diminishes the excretion of

urea (from 20 to 25 per cent.), reduces the rate of the pulsations, and lowers the temperature; effects which prove that oxidation is diminished and not increased. These results strictly accord with general experience. They likewise explain how certain substances, such as acid fruits, act as refrigerants; these fruits producing alkaline carbonates in the system, which diminish the temperature by diminishing oxidation.

The influence of section of the cervical Pneumogastrics upon the action of Emetics and Cathartics.—An extended series of experiments has been performed by Professor Horatio C. Wood (*American Journ. of the Med. Sciences*, July, 1870, p. 75) to solve the above problem. The solution he proposes may be briefly stated as follows: 1. In the majority of cases, division of the cervical pneumogastrics absolutely arrests free gastro-intestinal secretion, emetics and cathartics being powerless to produce it. 2. This arrest is not due to any direct influence which these nerves have upon the intestines or its circulation, but is owing to two or three other causes: accumulation of carbonic acid in the blood, interference with the circulation in the lungs causing congestion in the portal system, and possibly shock. 3. After division of these nerves the action of emetics and cathartics is completely prevented.

DR BRUNTON'S REPORT.

Nervous System.

Cause of Convulsions.—Hermann and Escher (*Pflüger's Arch.* III. 1, reviewed by Rosenthal in *Centralblatt*, p. 421) find that if the venæ cava and azygos be tied, and those in the vertebral canal stopped between the 2nd and 3rd thoracic vertebræ, convulsions occur just as if the arteries were tied, and were preceded by weaker respiratory movements if the animal were first rendered apnœic. The experiments were made on cats and did not succeed in rabbits. He is uncertain whether the convulsions are due to alteration of the brain-substance, or its irritation by products of decomposition in the blood, and thinks this is to be decided by passing a stream of some indifferent fluid through the vessels to remove these products. O. Nasse (*Centralblatt*, 1870, p. 273) has performed experiments of this sort; passing a ·65 per cent. solution of Na Cl into the vessels of an intestine, he found that movements which had begun in it after stopping its supply of blood were arrested. On passing a similar solution upwards into the abdominal aorta with a pressure of 120 mm. Hg, at the same time ligaturing the ascending aorta and opening the ventricle to avoid congestion, respiration continued for two minutes, but became slower and more superficial; in half a minute more reflex action was abolished, but there were no convulsions. He therefore concludes that the want of oxygen is no irritant to the nerve centres, and the effects which anæmia of them produces are due to the unremoved products of tissue change, especially CO_2, and perhaps to it alone. As an increase in the amount of oxygen in the blood dimi-

nishes nervous excitability (in apnœa, Rosenthal and Leube) and a lessened one increases it, Nasse thinks with Hermann that when the O is diminished, an amount of CO_2 which normally would have no effect, will act as an irritant, and produce the dyspnœa noticed by Pflüger when O in the blood was diminished without the CO_2 being increased.

Rosenthal thinks that this does not prove that the source of irritation is in the blood and not in the brain-substance, as automatic excitations are due to chemical processes in the ganglionic cells, and the total deprivation of nourishment when blood is replaced by Na Cl renders them incapable of action.

EPILEPSY.—Brown Séquard (*Arch. de Physiologie*, IV. 516). Incomplete section of the nates and cerebral peduncle of the right side produced spontaneous attacks of epilepsy, and these could again be excited by irritating the skin of the face and neck on the side opposite to the lesion, instead of the same side as when the fits were caused by section of the cord or the sciatic nerve. On laying bare the trunks of the nerves going to an epileptogene zone (*Arch.* 1869, p. 217), he found that the irritation of the wound caused the epileptogene power immediately to disappear, so that irritation of the skin no longer produced a convulsion.

In a guineapig, where one of the posterior columns of the cord was cut in the neck between the first and second pair of nerves, the epileptogene power was developed in nerves on both sides of the lesion, that is in the skin supplied by the trigeminus above it and in the 2nd, 3rd and 4th cervical pairs below it, the irritation being probably transmitted from the wound to a part of the nervous centre, and thence reflected to the skin, to which it gave an epileptogene power.

CAUSE OF CLONIC CONVULSIONS.—Nothnagel (*Virchow's Archiv*, XLIX. 267) thinks that the epileptiform convulsions which occur when a mammal is decapitated, and which again occur, though weaker, when a cut is afterwards made through the cord, are due to reflex action produced through the grey substance of the cord, as anæmia of the cord does not give rise to movement, and mechanical irritation has no effect on the anterior columns. When the central end of the divided sciatic nerve in a frog is irritated by an induced current shortly after decapitation, movements occur in the other leg only on the application and removal of the electrodes, the leg remaining quiet during the continuance of the stream; but when this was done 24 hours after, clonic contractions occurred, shewing that *continuous irritation of a peripheral nerve produces reflexly clonic contractions.* He explains the increased irritability after 24 hours, as well as the diminished sensibility of the skin shortly after division of the cord, by supposing that there are inhibitory centres in the cord whose irritability disappears sooner than that of the reflex centres.

INHIBITORY NERVES IN SKIN.—Fick and Erlenmeyer (*Pflüger's Archiv*, III. 326) found that if an isolated nerve of the skin be irritated, or if the skin itself be strongly irritated by concentrated

mineral acids, tetanic contraction of single muscles was produced instead of the ordinary co-ordinate movements which usually follow irritation of the skin. Fick thinks that inhibitory fibres which prevent the irritation from acting on co-ordinate reflex centres proceed from the skin as well as exciting fibres, but the former are only brought into action by strong irritation of the skin or direct irritation of the nerve itself.

NON-IRRITABILITY OF THE ANTERIOR COLUMNS OF THE CORD.—Huizinga (*Pflüger's Archiv.* III. 81) and Aladoff (*Bull. de l'Acad. de St Petersburg*) under E. Cyon's direction, found that mechanical and electrical irritation of the anterior columns had no effect unless the anterior roots were likewise irritated or the posterior columns and grey substance imperfectly removed. If any trace of the grey substance remained, contractions were produced, and Aladoff therefore thinks it irritable though the anterior columns are not.

ON THE WARMING OF NERVES AND NERVE-CENTRES BY IRRITATION OF SENSORY AND MOTOR NERVES (Schiff, *Arch. de Physiologie*, III. 323; and IV. 451).

LATENT PERIOD OF THE VAGUS.—Donders (*Proces-verbaal Ak. v. Wetenschappen te Amsterdam*, 1869-70, No. 2. *Centralblatt*, 408).—If the vagus of a rabbit be irritated by a shock from an induction coil, there is a latent period of ·17 of a second, so that if the next heart-beat would naturally take place within this period it is not retarded. If the shock occurs earlier, retardation takes place, and increases quickly at first, and then slowly, till the time between the shock and that of the next natural beat would be ·43 of a second, and then decreases till it is ·53, which is equal to a heart period ·36 plus the latent period ·17. Opening and shutting a constant stream has a similar but weaker action, and the latent period is ·203. The action of ascending and descending currents corresponds with Pflüger's law. When a constant current is passed through the nerve and it is then irritated by an induced current, irritability is markedly diminished at the positive pole but not increased at the negative, and there is no action on the heart corresponding to Ritter's tetanus in muscles.

RELATION OF TACTILE POWER TO THE MOBILITY OF PARTS.—Kollenkamp and Ullrich (*Zeitschrift f. Biologie*, VI. 37) at Vierordt's desire made experiments on the tactile sense in the arm, which confirmed and extended in detail those of E. H. Weber. From these Vierordt deduces the law (Pflüger's *Archiv*, II. 297, and *Zeitsch. f. Biol.* VI. 53) that the delicacy of touch in a part of the skin is the product of two factors; one variable, being proportional to its distance from a joint or amount of mobility when the joint is in action; and the other constant, being the tactile delicacy of the skin immediately over the axis of the joint itself.

ON TROPHIC NERVES.—Rolleston (*Quarterly Journal of Science*), p. 200.

ON THE NERVES OF DEGLUTITION.—Waller and Prevost (*Arch. de Physiologie*, III. 343).

ON ELECTROTONUS.—Goldzieher (*Pflüger's Arch.* IV. p. 240).

RAPIDITY OF CONDUCTION IN MOTOR NERVES OF MAN.—Place (*Pflüger's Arch.* VII. 424).

Ear.

FUNCTION OF THE SEMICIRCULAR CANALS.—Goltz (*Pflüger's Arch.* 1870, III. 172) repeated Flouren's experiments on the destruction of the semicircular canals in birds and rabbits, and Brown Séquard's on the section of the acoustic nerve in frogs, and obtained the same peculiar movements and loss of the power of balancing. He believes that the pressure of the endolymph on one side or other of the semicircular canals, varying with each motion, enables the animal to judge of the position of its head, and that sight and muscular sense are insufficient for this purpose when the endolymph has escaped, and that the peculiar movements are caused by its efforts to get its head again properly balanced. The movements of the body are only secondary, the animal being unable from the position of its head to judge of its relation to surrounding objects.

VIBRATIONS IN THE COCHLEA AND MOVEMENTS OF THE AUDITORY BONES.—The absence of Corti's rods in birds and amphibia led Hensen to suppose that the vibrations of different portions of the lamina spiralis corresponded to different tones. Helmholtz (*Heidelberg. Bericht*, V. 33) has investigated the possibility of different parts of such a membrane vibrating alone, and finds that they can if the longitudinal be very small compared with the transverse tension, and thus supports Hensen's hypothesis. Buck (*Heidelb. Ber.* V. 63), under Helmholtz's direction, found the vibrations of the auditory ossicles when the deepest tone of a syren was conducted into the ear were hardly visible and showed maxima at different pitches. The excursions were $\frac{1}{20}$, $\frac{1}{15}$ mm. The vibrations of the heads of the malleus and incus were parallel and nearly perpendicular to their axis of rotation, those of the stapes nearly perpendicular to its plane, but so that the whole stapes seemed raised.

Eye.

CIRCULATION IN THE EYE.—From experiments made in Donders' laboratory Dobrowolsky (*Centralblatt*, p. 305) found that the slightest pressure on the bulb of the eye caused emptying followed by dilatations of the veins surrounding the entrance of the optic nerve; and that their pulsation in the dog, though modified by all exertions, is essentially dependent on the pressure of the muscles of the eyes and eyelids, disappearing when these are divided or paralyzed, and has no connection with accommodation. Ordinary respiratory movements do not affect it, but the veins swell when these are impeded or stopped. In the human eye accommodation for near objects causes diminution, and for distant ones enlargement of the veins. The venous pulse differs from that in the dog; for while the latter lasts about six seconds, the former corresponds to the beats of the heart, the veins diminishing just before the radial artery expands. Donders thinks this pulsation in man is due to the pressure on the veins being increased by the expansion of the arteries at each cardiac pulsation.

Papers on Accommodation by Adamuk.—*Centralblatt*, 292, and Woinow, reviewed in *Centralblatt*, 478.——On Movement of the Iris, by Engelhardt, *Würzburg. Untersuch.* iv. 296, see *Centralblatt*, 478.——On Intermittent Irritation of the Retina. *Pflüger's Arch.* iv. 214.——On Taste. Schiff reviewed in *Centralblatt*, 330.

Vascular System.

First Tone of the Heart.—O. Bayer (*Arch. f. Heilk.* 1820, 157) passed water under different pressures into the apex of the heart, and found that when the tension was suddenly increased the auriculo-ventricular valves produced a tone if the heart were healthy, but any insufficiency of the valves, whether due to alterations in them or the muscular substance of the heart, invariably prevented it. He considers that this tone differs in its nature from the first tone of the heart, which he holds with Ludwig and Dogiel to be muscular.

Action of Respiration on Circulation.—Hering and Horwath (*Wien. Sitz. Ber. Math. Nat.* Cl. 2 Ab. i. 829) think the oscillations in arterial pressure noticed when an animal is curarized, and artificial respiration stopped, are due to oscillations in the innervation of the vaso-motor system which is not paralyzed by curara, and that they are co-ordinated with respiration (*Mitbewegungen*), as they correspond with the traces of respiratory movements or twitches in limbs which occur when the animal is imperfectly curarized. They are independent of the heart, as they occurred when a ligature separated it from the vessels, or even when it was replaced by a small pumping apparatus.

Circulation in Muscle.—W. Sadler (*Ludwig's Arbeiten*, 1870). The rapidity of the circulation in muscles under different conditions was measured by the amount of blood which flowed in a given time from the vein coming from it. The muscles used were the flexor carpi (*Handbeuger*) and the biceps femoris. The animals were narcotized with morphia or curara. In the normal muscle at rest the circulation is extremely slow, the amount of blood flowing from its vein being many times less than that from a skin-vein of similar size. When the nerve is divided no change is produced in the circulation in either muscle if the animal be narcotized with curara. If morphia be employed it remains likewise unchanged in the flexor carpi, but is increased in the biceps. The cause of this he is as yet unable to decide. The results of tetanizing the nerve were the same in both muscles, but differed according to the narcotic. When morphia was used, irritation of the nerve, either intermittent or tetanic, generally produced an increase in the flow, which sometimes attained its maximum during the contraction of the muscle, sometimes not till after it had again relaxed, but in no case did the rapidity remain long the same, but constantly varied even when the muscle remained in a state of tetanus. The same was observed when the muscle was prevented from contracting while the nerve was irritated, thus showing that the quickened stream was a result of the dilatation of the arteries and not connected with any change in the form of the muscle. It is difficult to explain these phenomena by the action of dilating and contracting vaso motor-nerves; it can be done much more easily by supposing that the muscular walls

of the vessels can be made to contract by the action of O on them, and this Ludwig prefers to do. This hypothesis is strengthened by the fact that when the animal was poisoned by curara—which, while paralyzing the motor, leaves the vascular nerves intact—irritation of the nerve produced no alteration in the blood-stream; it remained on the whole unaltered so long as artificial respiration was well kept up. As soon as it was suspended, however, the rapidity of the stream increased, again becoming slower when respiration was renewed and the blood regained its arterial colour. A somewhat similar fact was observed in a detached muscle (not poisoned) through which an artificial stream of blood was passed. During tetanus there was generally a diminution in the stream, after it an increase lasting for some time. The oftener that tetanus and rest alternate the slower becomes the stream, but it may be again greatly quickened by stopping the supply of blood for half an hour, and thus producing local suffocation of the muscle. The colour of the blood in the experiments with morphia is also noteworthy, as that which streams rapidly out after the tetanus is over is very dark, while that at the beginning of tetanus is often clear red like that in the vein of an irritated salivary gland. The author concludes that while this latter fact shows that the above-mentioned explanation is not universally applicable, the former tends to support it; and draws particular attention to the importance of the slow stream in the muscle at rest, its being washed out with blood after each contraction, and the great consumption of O, for the tissue-change and temperature in muscle, and of the sudden alteration of the circulation in muscle for that going on in other vascular districts.

INFLUENCE OF CLOSURE OF THE CAROTID ON THE CIRCULATION.—Nawalichin (*Centralblatt*, 483). In curarized animals (cats) with divided vagi and sympathetics, ligature of one carotid has comparatively little action on the circulation, but ligature of both causes a quickened pulse, and an increase in the blood pressure (60 per cent.) equal to that produced by compression of the aorta below the diaphragm, or irritation of the spinal cord after division in the neck. This is probably due to irritation of the vaso-motor centres and consequent contraction of the arteries in the body, as it does not occur if the spinal cord be previously divided.

LANDOIS (*Centralblatt*, 433) describes a gas sphygmoscope consisting of a shallow metal trough 6 centimetres long by 1 broad, and having a small pipe connected with each end. One of these is knee-shaped and ends in a point. Gas is passed through the apparatus and lighted at the point of the tube, and the variations in the size of the flame correspond to the beats of the pulse.

CONGESTION AFTER LIGATURE OF ARTERIES.—Brown Séquard believes (*Arch. de Physiol.* IV. 518) that the congestion observed in the kidney, spleen and part of the intestine, when the arteries supplying them are tied, is due to the destruction of the vaso-motor nerves running along the arterial walls, and consequent dilatation of the small vessels which they supply by a reflux of venous blood. In other organs where there is a certain amount of collateral circulation,

no reflux of blood takes place from the veins, but the supply from the arteries being diminished while the paralyzed arteries are larger than usual, it stagnates in them and becomes charged with CO_2. This he thinks is the cause of convulsions noticed after ligature of one carotid trunk in man.

ON THE PRODUCTION OF ŒDEMA.—Ranvier (*Compt. Rend.*, 1869) finds that obstruction to venous circulation is not sufficient to produce œdema if the vaso-motor nerves remain uninjured. Ligature of the vena cava inferior below the diaphragm in a dog causes coldness of the posterior extremities, but no œdema. If one sciatic nerve be cut, the vessels of the corresponding foot become full, the foot warm, and œdema appears in about an hour.

That this effect is not due to section of the motor nerves of the muscles, but to that of the vaso-motor fibres running in the sciatic, is shewn by the fact that when the vena cava was tied, the spinal canal opened, and the last three roots of the lumbar, and all those of the sacral nerves were divided on one side in one dog, and the cord divided above the lumbar enlargement in another, complete paralysis was produced; but in neither case did œdema occur, though the animals survived 15 and 20 hours respectively.

ON THE CIRCULATION IN INFLAMMATION.—Riegel (*Centralblatt*, p. 450) used as a standard of comparison for the rapidity of the circulation an artificial stream passing through the eyepiece of the microscope. He found that if the sciatic nerve were divided in a frog no effect was produced, but if the central end were irritated by an induced current, the rapidity of the circulation increased and the lumen of the arteries diminished up to a certain point, at which they remained nearly constant so long as the current lasted, and then returned to their normal, when it was discontinued; more slowly, however, if it had lasted long. Irritation of the nerve never caused contraction, but sometimes dilatation of the veins. If the current lasted long, oscillation in the calibre of the arteries was observed both during its continuance and after its stoppage. Application of croton oil causes first contraction of the arteries and rapid circulation; this gives place gradually to dilatation, and the stream returns to its normal rapidity, and then becomes slow, and the large veins dilate. Slowing begins in the capillaries and small veins, in some of which stasis may be present, while the rest of the circulation is rapid. Division of the sciatic seems to diminish the primary quickening and somewhat retards stasis, while irritation of its central end retards the occurrence of slowing and stasis to a very great degree.

Stricker and Norris (*Studien aus dem Inst. f. expt. Pathol. zu Wien*), in a most interesting research on inflammation, found that three hours after the application of nitrate of silver to the cornea of a frog, the branched cornea corpuscles were enlarged, granulated, and had a double nucleus. Sluggish movements were seen, and made more active by passing a constant stream of serum over the cornea. A few hours after, clumps of cells are seen with numerous nuclei and some with vacuoles. In 15—20 hours the cornea corpuscles have

8—10 nuclei; one often flat, the others small, round, and moveable, like white corpuscles (*Wandercellen*). The branched corpuscles then disappear and are succeeded by bodies with numerous nuclei, which they think are a transition to white corpuscles. They thus think that pus corpuscles are formed from the tissues in the part, and do not simply wander out of the vessels. Their non-formation in the tissues of the frog after replacement of the blood by Na Cl solution they think due to the disturbance of nutrition.

Stricker saw the colourless corpuscles in the inflamed tongue of a frog become fissured and then divide. The secondary cells thus formed became also fissured, but he did not see them divide. He also observed in the connective tissue corpuscles of the tongue a similar fissure but no division. Complete division was however noticed in an epithelial cell in an inflamed cornea. To shew that the alteration in cells in inflammation is due to the alteration in their nutritive supply, he placed a healthy cornea in the same conditions as an irritated one by cutting it out, placing it over an irritated one, and sewing up the eyelid over it. In 24 hours it too was inflamed, and contained pus corpuscles. That these were formed from the tissue and had not wandered in, was shown by washing one half of a cornea with distilled water which would alter its nutrition but present no obstacle to the white cells, and then treating as before. The washed half contained few or no white cells, while in the other half they were abundant.

Picot, "On Inflammation," *Compt. Rend.* LXX. 1367.——Heller, "On Inflammation," *Centralblatt*, 310.——Lorain, "On effects of Blood-letting," *Jour. d'Anat.* IV. 337.

Tschaussow (*Centralblatt*, 305, and 402) saw in the web of a frog, where the circulation had returned nearly to its normal condition after irritation 7 days before, a pigment-cell in a vein adhere to its wall, slowly creep through it and then move about outside. While moving about it divided into two, each of which moved like the first.

Respiratory System.

RESPIRATION IN FISHES.—Gréhant (*Bibl. de l'Ec. des hautes études, Sect. d. Sc. Nat.*, 1869, 299) finds that fishes in a limited amount of water take up all or nearly all its O, and give out a greater amount of CO_2, sometimes twice as much. Nitrogen is sometimes taken up, sometimes given off, but if the swimming-bladder be extirpated it remains unchanged. Gréhant, "On absorption of CO," *Compt. Rend.* LXX. 1182.

ACTION OF DIFFERENT GASES ON RESPIRATION.—Berns (*Nederl. Archiv.* V. 179; *Centralblatt*, 388) thinks CO_2 acts as an exciter of respiration directly on the vagus ends in the lung, and also in the blood. The first action is absent when the vagi are rendered inactive by apnœa.——"ON RESPIRATION IN SHEEP," Henneberg, Schulze, Mäcker, and Brun, *Centralblatt*, 353 and 369.

Secretion.

ACTION OF THE SYMPATHETIC ON THE SECRETION OF URINE.—M. Peyrani (*Comp. Rend.* LXX. 1300) estimated the urine before, during, and after irritation of the sympathetic in the neck both cut and uncut, with constant and induced currents of all strengths, and concludes: 1. That the amount of urine and urea increases in proportion to the strength of the current. 2. When currents of the same strength are used, the induced increases the secretion of urea and urine much more than the constant current. 3. When the sympathetic is cut but not irritated the urine and urea sink to a minimum. 4. When the peripheral end of the cut sympathetic is galvanized, the amount of urine and urea ascends much above the normal, but is always below that obtained by galvanizing it before cutting.

EXCRETION OF UREA BY THE KIDNEYS.—Gréhant (*Bibl. de l'Ec. des hautes études, Sec. d. Sc. Nat.* I. 265) found after extirpation of the kidneys or ligature of both ureters (in dogs), that the amount of urea in the blood was greater than normal, and increased steadily after the operation. He thinks that ligature of the ureters, by causing distention of the pelvis of the kidney and stoppage of the circulation, destroys the functional activity as much as extirpation. Comparing the blood of the renal artery and vein, he found that the blood of the renal vein constantly contained less urea than that of the artery, the difference being greater when circulation is slow, but after ligature of the ureters the amount in the venous blood was exactly the same as in the arterial. He therefore concludes that the kidneys excrete but do not form urea.

HYDRURIA.—Eckhard (*Beiträge*, VI., *Heft* I. p. 1) finds that hydruria produced by the inhalation of CO (Schmiedeberg's method) has the same characters as that produced by puncture of the floor of the fourth ventricle. The urine is much clearer and more abundant than that secreted after simple section of the splanchnics. This secretion, though independent of section of the splanchnic, is greatly increased by its division previous to the inhalation of CO, and can be completely stopped by its irritation. Irritation of the exposed spinal cord after division of the vagi and the splanchnic lessened the secretion, which rose slowly afterwards, and did not attain its normal. As this might occur through a reflex inhibition of the exciting centre nerves, it is not certain whether other inhibitory peripherical nerves than the splanchnic exist or not.

The hydruria has no connection with the vago-sympathetic trunk. It was often produced after division of both splanchnics and all nerves going from the sympathetic cord to the kidneys. Attempts to produce hydruria after section of the spinal cord gave no positive result; when it had been already produced, division of the cord at the sixth cervical vertebra stopped it. It is therefore probable that CO produces it by irritation of a nerve centre above that point.

COMPOSITION OF URINE AFTER SECTION OF THE SPLANCHNICS.—Knoll (*Eckhard's Beiträge*, VI. *Heft* I. 41) finds that when the splanch-

nic is cut on one side the urine secreted by that kidney as compared with that of the other one whose nerves are intact, has a specific gravity which is somewhat, but not much, lower, and has no direct relation to the increase of urine. Its solids, and especially urea, though less relatively to its water, are absolutely greater; and this absolute increase is larger, the greater the increase in urine secreted. Albumen appears in the urine, and its reaction changes, but these are not due to division of the splanchnic, as they sometimes appear previously. The change from acid to alkaline is due in part at least to a fixed alkali.

ORIGIN OF DIABETES.—Lusk (*New York Med. Journal*, July 1870) undertook a series of experiments with the view of confirming Pavy's, but came instead to the following conclusions. The blood of the general system in carnivorous animals confined to a nitrogeneous diet contains appreciable quantities of glucose, not only during digestion, but even after fasting a considerable time. Under strictly physiological conditions the blood of the right side of the heart contains from $\frac{1}{4}$ to $\frac{1}{2}$ a grain of glucose per fluid ounce, a quantity 2 to 4 times greater than that in the jugular vein. This indicates a considerable amount of sugar in the pure hepatic blood; and although in diabetes the sources of sugar may be various, the chief one is the liver. In a foot-note he notices two cases of diabetes in which sugar disappeared from the urine after inhalation of oxygen.

Eckhard (*Beiträge*, VI. *Heft* I. p. 22), while expressing his belief that the study of diabetes occurring after curara poisoning can throw very little light on the pathology of the disease, gives as his experience that when a solution of curara of about 2 per cent. is injected in doses of ·5—5 Cc (best 1·5 Cc), a well-marked diabetes sooner or later invariably appears both in dogs and rabbits. It generally lasts only a short time, and will not be perceived if the experiment be not long enough continued (one or two hours). It may continue after the effect of the curara has passed off, and voluntary respiration begun. He believes that the occurrence of diabetes after extirpation of the superior cervical and semilunar (cœliac) ganglia is not due to their *absence*, but to some other cause, as he has repeatedly extirpated the superior cervical ganglion, and diabetes never occurred, but he has found it after simple wounds. After extirpation of the semilunar ganglion it was only in rare cases that sugar was plainly found in the urine, while after operations near the ganglion which did not injure it, sugar was present in considerable quantity. He had come to the conclusion (*Beit.* IV. p. 11) that puncture of the fourth ventrical produced diabetes through the splanchnics, but found an opposite statement by Fritz (*Gaz. Hebd.* 1859, 264, 294, 344, 374). As he could not discover anything about the experiments on which this statement rested, he did not repeat his own, but took up the question whether, in general, diabetes is possible after section of both splanchnics? For this purpose he used Schmiedeberg's method of producing diabetes by inhalation of CO, and found that in dogs it produced diabetes after section of both splanchnics. In rabbits it did not produce it either after or before section of the splanchnics.

Miscellaneous.

Engelmann (*Pflüger's Arch.* II. 243; and *Nederl. Arch. von Genees en Naturkunde*, v. 1) finds that the fresh ureter appears homogeneous, the cells composing it only becoming visible when it is dying. Peristaltic movements continue in pieces which have no ganglion cells. Contractions can only be produced by direct irritation of the muscular substance, and not of the ganglia or adventitia, and they will not pass over an injured part. He therefore considers the whole ureter as physiologically one structure; the wave of contraction passes from one part to another without the intervention of nerves just as it would in a single involuntary muscular fibre. Ciliary movement he thinks is also propagated in this way, the fresh cells being continuous with one another. He considers the movements as automatic, that is, occurring without visible external irritation like amœboid movements in the cells.

Engelmann, ON TETANUS, ON OPENING AND SHUTTING, *Pflüger's Arch.* III. 403.——ON NEGATIVE OSCILLATION IN MUSCLE, Lamansky, *Pflüger's Arch.* III. 193.——PRE-EXISTENCE OF A CURRENT IN MUSCLE, J. Worm Müller, *Würz. Untersuchungen*, IV. 181.——Hermann, ON THE CAUSES OF ELECTROMOTOR APPEARANCES IN MUSCLES, *Pflüger's Arch.* III. 1.——Schiff, ON THE NON-EXISTENCE OF ELECTRIC CURRENTS IN NERVE, reviewed in *Centralblatt*, 421.——ON EXHAUSTION IN MUSCLE, Volkmann, *Pflüger's Arch.* VII. 372.——Schenk, ACTION OF COLD ON ELEMENTARY ORGANISMS, *Wien. Sitz. Ber. Math. Nat. Cl*, Abt. LX. 1869.——Engelmann, ON PROTOPLASM, *Pflüger's Arch.* II. 307.——Herwath, ACTION OF COLD, see *Centralblatt*, 414, and ON ANIMAL HEAT, 546.——Jacobson and Leyden, ON FEVER, *Centralblatt*, 259.——Simons, ON FALL OF TEMPERATURE, *Centralblatt*, 414.——Naunyn, ON FEVER, *Du Bois's Arch.* II. 159.——Duresk, ON THE PRODUCTION OF ARTIFICIAL INVERSION OF VISCERA BY HEATING DIFFERENT PARTS OF THE OVUM, *Compt. Rend.* LXX. 761.

DR GAMGEE'S REPORT.

PHYSIOLOGICAL CHEMISTRY.

Respiration.

RESPIRATION OF THE TISSUES.—In his *Leçons sur la Physiologie Comparée de la Respiration* (Paris, 1870), M. Paul Bert has in the 3rd and 4th chapters discussed at considerable length the important question of the respiration of the tissues; besides describing his own experiments he gives some interesting historical information, and specially discusses the views which have of late years been brought forward by Professor Hermann.

In the first place the author insists that the credit of discovering the respiration of the tissues ought to be ascribed to Spallanzani, and not to George Liebig, as the former observer, instead of restricting his attention to one tissue, examined many.

"In so far as this is concerned," says Bert, "the otherwise remarkable work of G. Liebig, so far from having introduced any new fact

into science, has remained much behind what Spallanzani had so long before demonstrated."

The materials used in the author's researches on the respiration of the tissues were obtained from animals which immediately before had been bled to death. The portions of tissue to be examined were cut into pieces, one centimetre cube, which were suspended on little copper gratings introduced into absorption tubes standing over mercury and containing air. The weight of tissue and the volume of air were constant (?) in all these experiments, the weight of tissue being always 50 grammes and the volume of air 180 cubic centimetres. The following are the results of an experiment in which various tissues of a dog were exposed to the action of air for 24 hours, the temperature varying during that period from 10°C. to 0°C.

			cc.			cc.	
100 grammes	of muscle	absorbed	50·8	of O,	and exhaled	56·8	of CO_2.
"	" brain	"	45·8	"	"	42·8	"
"	" kidney	"	37·0	"	"	15·6	"
"	" spleen	"	27·3	"	"	15·4	"
"	" testicle	"	18·3	"	"	27·5	"
"	" crushed bones with marrow		17·2	"	"	8·1	"

In a second experiment with the tissues of a dog the following numbers were obtained:

			cc.			cc.	
100 grammes	of muscle	absorbed	53·0	of O,	and exhaled	39·8	of CO_2.
"	" kidney	"	21·8	"	"	34·2	"
"	" spleen	"	13·9	"	"	26·6	"
"	" crushed bones with adhering marrow		10·6	"	"	12·6	"

In another set of experiments Bert took defibrinated and thoroughly arterialized blood of the dog, and placed in contact with it for four hours pieces of muscle and spleen. The quantity of blood used in each experiment was 60 grammes; of muscle and spleen 46 grammes.

After the four hours had expired, the blood which had been in contact with the tissues was analyzed by the method proposed by Claude Bernard for the estimation of oxygen, *i.e.* a given volume of blood was agitated with carbonic oxide, and the quantity of oxygen and CO_2 which had been displaced from the blood determined by the analysis of the gas.

	O.	CO_2.
The 60 cubic cents of blood with which the muscle had been in contact evolved	3·6	0·4
The 60 cubic cents of blood with which the spleen had been in contact	7·3	0·9
60 cubic cents of blood left intact	10·7	0·5

The author then investigates the gaseous interchanges which take place between the tissues and atmospheres of varying composition.

He corroborates by his own experiments the statement of Spallanzani, that the richer the gas in contact with a tissue is in oxygen, the greater is the quantity of oxygen absorbed.

With atmospheres of identical composition and with tissues of the same nature, but derived from animals of different species, there are, according to Bert, the greatest differences in the amount of oxygen absorbed; thus he finds that the muscles of cold-blooded vertebrate animals consume relatively to their weight less oxygen and evolve less carbonic acid than muscles of warm-blooded animals.

	O.		CO_2.
100 grms of muscles of a cock absorbed	62 cc.,	and evolved	54·6.
" " frogs "	42·5 "	"	36·6.

He further states that the muscles of adult animals absorb much more oxygen and evolve more carbonic acid than those of young animals; thus,

	O.		CO_2.
100 grms of muscle of adult dog absorbed	47·3 cc.	and exhaled	56·3 cc
" " new-born dog "	29·3 "	"	35·7 "

The author believes that these experiments throw some light on the fact that newly-born animals resist asphyxia much longer than adult animals.

Having proved that all the tissues of the body absorb oxygen, Bert inquires whether we are justified in looking upon this absorption of oxygen by muscle as a true respiration or as the commencement of a decomposition, which, as Hermann would have it, sets in at the surface of a muscle exposed to air and propagates itself so as to involve the whole muscle—a decomposition which indicates that the unstable equilibrium which constitutes life is broken, and that the organized matter has as it were commenced to form part of the inorganic world?

The doctrine that the muscle separated from the body becomes immediately, from one point to another, from the surface downwards, the seat of death and putrefaction, is opposed most strongly by Bert, who argues in favour of a respiration of muscle, in the sense in which it was understood by Spallanzani and George Liebig. Referring to the hypothesis that the muscle which is removed from the body commences at once to die, he remarks: "How could I accept it (the hypothesis) after having seen complex animal structures, entire limbs with their bones, their cartilages and their nerves, which have been separated not merely for a period of some minutes or of some hours, but for four, six, and eight days, continue, after the conditions necessary to their existence have been restored to them by grafting, to live, to grow, to complete their evolution, to undergo waxy, fatty and atrophic degenerations, in a word to be healthy or to become diseased, and disease is a proof of life?

"The air of the tubes in which I preserved these structures had become poor in oxygen, and loaded with carbonic acid. Had putrefaction occurred there? If so, we must deny to this term its universally accepted meaning, and declare that putrefaction is one of the phenomena of life."

The sentences we have just quoted, in which M. Paul Bert refers to his own very interesting experiments, appear to us as the most important in the two chapters in which he discusses the respiration of the tissues.

M. Bert's remarks on the dangers which are to be anticipated from over refinement of the methods of research used in physiological investigation, *i.e.* on the errors which often creep in when all is sacrificed to an unnecessary accuracy in individual experiments, whilst they prove the author to be an able writer, appear to us to lose much of their force from the very obvious jealousy and animosity with which he criticizes his physiological brethren "de l'autre côté du Rhin." M. Paul Bert, when he penned his remarks on this subject, was obviously thinking of his great master, Claude Bernard, who, with comparatively crude methods, obtained remarkably accurate results in all the departments of physiology to which he directed his attention. His work should not, however, cause a sneer to be cast at the splendid school which has for its leaders such men as Ludwig and Helmholtz and Du Bois Reymond.

RESPIRATION IN THE LUNGS.—Dr J. Müller has under Ludwig's direction (Ludwig's *Arbeiten*, 1870) performed a series of experiments in order to ascertain the changes which the gases of the blood undergo in the passage of this fluid through the lungs.

An artificial stream of defibrinated blood at the ordinary temperature of the room (18° C. or 20° C.) was passed through the lungs, a canula being placed in the pulmonary artery and others in the pulmonary veins. The lungs were inclosed in a perfectly tight caoutchouc bag, which was inserted into the edges of a metallic plate, through which passed the canulæ conducting blood to and from the lungs, a canula which communicated with the trachea, and another which communicated with the caoutchouc bag. Both the trachea and bag were filled with nitrogen, and had manometers connected with them. As in the researches with an artificial current of blood passing though muscle (Ludwig and Schmidt), it was found that in order to keep up a constant rate of flow 2—3 cc. per minute the original pressure had to be increased from 5 to 20 or 30 mm. of mercury. Arterial blood which had flowed through the lung always became of a dark venous colour, and a very marked decrease in the amount of oxygen was always noticed. The amount of oxygen consumed increases with the rapidity of the stream and the amount of oxygen contained in the blood, and with the length of time which has elapsed since the commencement of the experiment. The amount of carbonic acid evolved is nearly constant; the quantity increasing with the rapidity of the stream, although not so decidedly as the oxygen consumption. The average value of the quotient $\frac{O}{CO_2}$ is 2.

The simple nature of the lung-tissue rendered it very suitable for a research such as that of Schiremetjewski (*Journal of Anatomy*, Nov. 1869), to determine the capability of oxidation possessed by the blood. Lactate of sodium was added to the circulating blood, and the amount of oxygen and carbonic acid determined both before and

after the passage through the blood-vessels of the lung. If the lactate of sodium had undergone combustion, the volume of CO_2 produced should have been equal to the oxygen consumed, but instead of this it was found to be four to six times as great, showing that lactic acid must split up in the blood, much in the same way as it does in the butyric acid fermentation, where CO_2 is evolved independently of oxidation; $2C_3H_6O_3 = C_4H_8O_2 + 2CO_2 + 2H_2$. We can understand the dependence of the quotient $\frac{O}{CO_2}$ upon the rapidity of the stream, as the oxidations following the decomposition will be much influenced by the amount of available oxygen, which of course must depend much upon the rate of flow. To determine whether the lung possesses a specific functional power of excreting CO_2, the blood of an asphyxiated animal was passed alternately through a lung inflated with N, and a simple space filled with the same gas, both the cavities filled with gas being connected by means of a differential manometer. As the experiment went on, the pressure, which was at first alike in both, became greater in the lung-cavity, showing that the lung possesses a specific power of excreting CO_2. During this experiment it was observed that the blood-stream passed with much greater difficulty through the inflated than the collapsed lungs.

It would appear to the reporter that this experiment scarcely points to any *specific action* on the part of the lung-tissue; as it would be expected that in circulating through the capillary network of the alveoli the blood would be placed in the best possible condition to allow the carbonic acid to diffuse into the N atmosphere, a condition which could scarcely be effectually imitated by any artificial arrangement.

ON THE RELATION BETWEEN THE OXYGEN ABSORBED DURING THE DAY AND NIGHT.—In an original paper published lately (*Centralblatt für die Medicinische Wissenschaften*, 1870, Nos. 23 and 24), Henneberg, Schultze, Märker and Büsse give the results of experiments performed on two full-grown sheep in a Pettenkofer's chamber. Tables accompany their paper, in which, *inter alia*, are noted the temperature of the room, the amount of hay consumed, the quantities of carbonic acid, marsh gas, and watery vapour excreted, and the relation of the quantities of oxygen absorbed during the day and night to the quantity of carbonic acid.

The quantity of carbonic acid, excreted during the day and night periods into which the period of 24 hours was divided, differed. The variation seemed to depend upon the quantity of food taken in the two periods. There was a constant relation between the quantity of hay added to the diet, and the increase of carbonic acid. For each gramme of hay carbonic acid was excreted to the extent of 0·11 to 0·12 grms. Between the excretion of carbonic acid and water there was a very marked parallelism; the greater the one, the greater the other.

The absorption of oxygen and the excretion of products containing oxygen did not always proceed *pari passu*. The changes in this respect were not of a symmetrical character corresponding with

either the day or night period. Abstinence from food for 24 hours caused the carbonic acid and aqueous vapour to sink to one-half.

Blood.

PROOF THAT THE BLOOD-CORPUSCLES YIELD FIBRIN.—Heynsius and Von der Horst shewed, some time since, that the stroma of the blood-corpuscles yields to a salt solution a body whose properties correspond with those of fibrin. Heynsius also shewed (*Pflüger's Archiv*, 1869), that blood which immediately after leaving the blood-vessels is treated with a solution of phosphate of sodium, and which therefore coagulates slowly, yields more fibrin than blood not treated in this manner, the quantity of fibrin being sometimes doubled. Lastly, he shewed that when blood is mixed directly with a solution of $Na\,Cl$ cooled to 0° C., the very dilute plasma contains much less fibrinogen than corresponds to the quantity of fibrin which would be obtained from the same blood if allowed to coagulate spontaneously. The direct proof that fibrin does really, in part at least, proceed from the blood-corpuscles was not, however, forthcoming. In a paper published in the *Centralblatt* (No. 25, 4th June, 1870), Heynsius puts us in possession of facts which abundantly prove the accuracy of his previous conjectures. This observer had previously discovered that corpuscles which have been separated by the addition of a solution of salt cooled to 0° C. are incapable of inducing fresh coagulation when added to serum. He sought for an explanation of this fact in the hypothesis that the stroma of the blood-corpuscles had been coagulated in the process adopted for their separation

The blood of the horse differs from that of other animals in its very slow coagulation, so that when exposed to great cold it only coagulates after many hours. Might not the coagulating power of the corpuscles be abundantly proved by experiments with this blood? The result thoroughly justified this surmise which Heynsius made. He separated corpuscles from the blood of the horse by the addition of ten times its volume of a $Na\,Cl$ solution of 4, 3, and 2 per cent. In this way he obtained blood-corpuscles which, when mixed with water or with blood-serum, yielded a good blood-clot. The horse's blood contained 1·2 per cent. of fibrin, of which only 90 per cent. could be obtained after separating the blood-corpuscles. It is therefore evident that a portion of the fibrin of blood is derived from the blood-corpuscles.

ACTION OF PEROXIDE OF HYDROGEN IN BLOOD.—Schönn (*Centrallbatt*, 1870, No. 22) points out some fallacies in Schönbein's method of detecting HCN in blood by means of peroxide of hydrogen.

HAEMOCHROMOGEN.—Hoppe-Seyler in a preliminary notice (*Centralblatt*, 1870, No. 16) describes a new colouring matter which is obtained as a product of the decomposition of blood-colouring matter. Alcohol containing in solution SO_2 or KHO decomposes haemoglobin into an albuminous substance, and the new purple-red colouring matter to which the above name is given.

Hypoxanthin in the Blood of Leucocythæmia.—E. Reichardt (*Jenaische Zeitschrift f. Med. u. Naturk.* 1870, v. 389—392) describes the examination of the blood of a patient who was an inmate of the Jena Clinik, and who was 43 years old. There was simply enlargement of the spleen. The proportion of white to red corpuscles was as 1 : 2·22. Hypoxanthin as well as gluten could be readily detected in the blood. From 30 grammes of blood he obtained 0·123 grammes of gluten and 0·055 gram. of hypoxanthin. The author thinks he also detected a body called albukalin, which has been described by Theile; this substance is a product of the action of KHO on vitellin and albumen, and has the composition $C_4H_8NO_3$.

Digestion.

Professor Schiff (*Sunto dei lavori fatti nel Laboratorio Fisiologico di Firenze*, 1869. Prima parte. Digestione. Lo Sperimentale, 1870) has investigated many questions connected with the function of the bile in the process of digestion. He believes that a portion of the bile poured into the intestine is reabsorbed and contributes to the formation of new bile. He specially has examined the alleged precipitation of peptones from chyme by the bile, and he asserts that the process is very much influenced by the greater or less acidity of the chyme. When the chyme is very slightly acid, merely a precipitation of the bile mucus occurs; but when the acidity is considerable, the peptones are thrown down. When chyme enters the intestine its activity, as is well known, ceases. The cessation of activity occurs even before the pancreatic juice has acted upon it. Schiff finds that the secretion poured into a tied duodenum after the ligature of both the biliary and pancreatic ducts, possesses in a remarkable degree, the property of arresting the digestive action of the gastric juice; this secretion is alkaline and proceeds doubtless from Brünner's and Lieberkühn's glands. The author also states that after the extirpation of the spleen the pancreas no longer digests albuminous substances. (This abstract is taken from Professor Hermann's summary of Schiff's researches in the *Centralblatt*, 1870, No. 19.)

On the Decomposition of Fats in the small Intestine.—Brücke (*Wien Acad. Sitzungsber.* 2 Abthl. LXI. *Centralblatt*, March, 1870) has found that the chyle of recently killed dogs does not readily yield its fat to absolute alcohol, although ether readily extracts it. From this it would appear that the greater part of the fat contained in chyle consists of neutral fats and not of fluid fatty acids, as alcohol readily dissolves these. That a small quantity of the latter is always present, cannot, however, be doubted. What can be the use of the partial decomposition of neutral fats which goes on in the alimentary canal? Brücke finds that oil which contains free fatty acids is emulsionized by a solution of soda with much greater ease than pure oil. It would therefore appear that the fat-decomposing action of the pancreatic juice, causing the appearance of fatty acids which are converted into soaps by the alkaline juice, favours the emulsionizing, and therefore the absorption, of neutral fats. For some other observations on Peptones by Brücke, see *Centralblatt*, 23rd July, 1870, No. 33.

W. SCHMIDT (*Zeitschrift f. Chemie*, 1870) finds that when coagulated albuminous substances are heated with water in sealed tubes until solution has occurred, there is no production of bodies having the reaction of peptones.

DIGESTIBILITY OF CELLULOSE.—Weiske fed two individuals on a diet containing large quantities of cellulose (carrots, cabbage, and celery) and analyzed the ingesta and egesta. He found that in one case 62·7 per cent. and in another 47·3 per cent. of the cellulose was digested.

INFLUENCE OF THE SALTS CONTAINED IN FOOD ON THE COMPOSITION OF THE BONES.—Papillon has examined the modifications which occur in the bones of animals whose diet did not contain the normal salts present in food. (*Journal de l'Anatomie et de la Physiologie*, VI. 153—163.) He fed rats on sugar, gelatine, rice and water in which nitrate, chlorate, sulphate, and carbonate of potassium, and sodium were contained, in the proportion of 1·5 grammes per litre. All the animals fed in this manner died of inanition at the end of three weeks, and it was observed that the teeth had become as brittle as wood and were partially broken. Another rat was fed in the same manner as the preceding, with the addition daily of one decigramme (0·1 grm.) of phosphate of manganese; this animal died of acute enteritis in eight days. Another rat received daily, in addition to the first diet, a quantity of phosphate of aluminum dissolved in hydrochloric acid. In 73 days the animal died suddenly with convulsions, and post-mortem examination revealed severe enteritis. In 100 parts of the calcined bones there were—

Alumina	6·95
Lime	41·10

In another case phosphate of magnesium was added to the diet. The animal was killed after 70 days, and the analysis of the calcined bones gave the following result—

Magnesium	3·56
Calcium	46·15

Urine.

PRODUCTION OF UREA BY THE OXIDATION OF ALBUMINOUS SUBSTANCES.—Béchamp (*Comptus Rendus*, LXX. pp. 866—869) again asserts that he has obtained urea by oxidising albumen by means of permanganate of potash. He has left the matter quite undecided, as he did not subject to elementary analysis the substance which he considered to be urea.

PRESENCE OF HIPPURIC ACID, AND ABSENCE OF URIC ACID IN A CASE OF DIABETES INSIPIDUS.—(*Centralblatt*, 18th June, 1870.) Hofmann describes a case of diabetes insipidus, complicated by severe pruritus cutaneus, which occurred in Hebra's Clinique. The urine was very acid. It contained neither albumen nor sugar. There was

a complete absence of uric acid, but hippuric acid was present in abnormal amount. Total quantity of urine in 24 hours, 2500 c.c.

Urea	. . .	51·1 grammes.
Chloride of sodium	.	14·7 ,,
Phosphoric acid	.	5·4 ,,

URINE IN LEUCOCYTHÆMIA.—E. Reichardt, in a paper already quoted (*Centralblatt*, No. 22, 1870) states that in a very well-marked case of splenic leucocythæmia, where the white corpuscles were very much increased hypoxanthin could be readily separated from quantities of urine not larger than 400 c.c.——SALKOWSKY (*Virchow's Archives*, 1870, p. 174—210) has also investigated in the most critical manner a case of purely splenic leucocythæmia. In this case there was a remarkable and constant increase in the amount of uric acid; the uric acid excretion bore to that of urea the relation of 1 : 16·3. Using the very best methods, the author was unable to detect a trace of hypoxanthin in the urine, even when he employed as large a quantity as 3000 c.c. The following papers may also be consulted in reference to the urine; Jaffé, "On Urinary Pigment" (*Virchow's Archives*, XLVII. pp. 405—427); Thudicum, "On Kryptophanic Acid the Normal Free Acid of Urine" (*Centralblatt* 1870, Nos. 13 and 14). Naunyn, "On the Excretion of Urea in Fever" (*Berlin Clin. Wochenschrift*, 1869, No. 4).

NEW METHOD OF DETERMINING THE QUANTITY OF GRAPE SUGAR IN URINE.—Knapp (*Annalen Cl. Chemie u. Pharm.* LXXVIII. 252—254) has devised a new method for estimating the quantity of grape sugar, which is based on the fact that an alkaline solution of cyanide of mercury is completely reduced when boiled with a solution of grape sugar. The following abstract is taken from *Nature*, Oct. 13th, 1870:—"By direct experiment it was found that on boiling, 400 mgr. $Hg(CN)_2$ are reduced by 100 mgr. sugar. The solution is prepared by dissolving 10 grm. pure dry $Hg(CN)_2$ in water, adding 100 c.c. of a sodic hydrate solution of 1·145 sp. gr., and diluting to 1000 c.c. Pure grape sugar is prepared by recrystallizing the commercial dried at 100° from absolute alcohol. The experiment is performed by heating 40 c.c. of the mercury solution—this amount corresponds to 100 mgr. sugar—to boiling in a porcelain dish, and then adding sugar solution to complete precipitation of the mercury, the end of the reaction being ascertained by placing a drop of the liquid on to a piece of the finest Swedish filter paper, covering a small beaker containing some very strong ammonic sulphide; a brown spot is observed so long as mercury remains in solution. The advantages of this method over Fehling's are, that being equally accurate, the test solution is exceedingly easy to prepare and perfectly stable, a shorter time is required for the estimation, and that the foreign bodies which mask the pure colour of the cuprous oxide are without influence on the reduction of the mercuric cyanide."

NOTICES OF RECENT DUTCH AND SCANDINAVIAN CONTRIBUTIONS TO ANATOMICAL AND PHYSIOLOGICAL SCIENCE. By W. D. MOORE, M.D., Dub. et Cantab., M.R.I.A., &c. &c.

1. In the *Nederlandsch Archief voor Genees en Natuurkunde*, Vol. 5, an abstract is given of the description by T. Zaaijer of four anatomical irregularities observed by him:

(1) A case of *vena cava superior dextra et sinistra*. The right exhibited no peculiarities, the left had at first a diameter of 1·1 centimetres, ran in front of the left subclavian artery, crossed the arch of the aorta, passed the obliterated ductus venosus, and just in front of the left pulmonary artery took up the vena hemiazygos. The inferior part of the vein (ductus Cuvieri sinister) had a diameter of 1·6 centimetres, ran beneath the left pulmonary veins into the sulcus atrio-ventricularis, became suddenly much wider (2·7 centimetres), took up a vein issuing from the sulcus longitudinalis, and opened into the right auricle. The author gives the usual embryogenetic explanation of the origin of this anomaly.

(2) *Anomaly in the formation of the vena cava inferior*. In the body of a man aged 62 the right kidney was entirely wanting. The vena cava inferior lay in the usual manner with respect to the aorta, and arose at the normal height from the union of two branches, of which the left ran in front of the right common iliac artery, soon placed itself to the inside of the left common iliac artery, and took up the external iliac and hypogastric veins. Shortly before opening into the trunk of the vena cava this branch took up a transverse branch, which passed in front of the left common iliac artery and soon connected itself with another branch arising from the left common iliac vein, ran behind the corresponding artery, 1·5 centimetres to the left of the aorta, passed upwards and finally opened into the left renal artery. In this manner a triangular opening was formed, through which the common iliac artery was, as it were, pushed. The right branch (vena iliaca comm. dextra) ran first on the outside of the corresponding artery, soon passed behind it, and became connected by a transverse branch with a vein (vena hypogastrica dextra), which opened into the above-described left branch of the vena cava. There was thus formed a rounder opening for the arteria iliaca communis dextra.

The author looks upon the left branch running close to the aorta as the dilated left inferior cardinal vein. The transverse connecting branch may be considered as a vein corresponding to one of the lumbar veins. The position of the left common iliac vein in front of the right common iliac artery, and the opening of the right hypogastric vein into the left common iliac vein however defy all attempts at explanation.

(3) A *musculus radio-carpo-metacarpeus*. Under this name is described an anomalous muscle, arising from the radius, in some measure connected with the musculus pronator quadratus, and attaching itself by several slips to the ligamentum carpi transversum, the trapezium, and the bases of the 2nd, 3rd, and 4th metacarpal

bones. The author describes other peculiarities, and discusses the question whether Fano or Gruber has first described the muscle.

(4) A considerable anomaly of the first and second ribs of the right side with asymmetry of the manubrium sterni, which was much more highly developed on the left than on the right side. The first rib was connected with the sternum only by a fibrous cord, but by the thickened bony end movably with the second rib. A detailed description of the peculiarities, as well as of the points in which this differs from earlier observations of a similar nature, would exceed the limits of this report.

2. Professor DONDERS contributes to the same Journal an essay *On the action of the eyelids in determination of blood from expiratory effort.* The investigations there detailed owed their origin to a request preferred by the celebrated Mr Charles Darwin, that Professor Donders would give him in writing his opinion as to the statement of Sir Charles Bell, "that pressure of the eyelids protects the eyes against the injurious influence of determination of blood in violent or persistent expiratory efforts." The author enters at considerable length into an examination of the condition, in violent expiration, of the external vessels of the eye, as well as of the intra-ocular, and of the retro-ocular vessels; establishes by direct experiment the fact that in violent expiratory efforts kept up for a time, while the face becomes red and swollen, the eyeball distinctly moves forward. He also confirmed Dr J. J. Müller's observation, that in voluntary widening of the slit of the eyelids the eyeball advances, and he found, in addition, that in nictitation the eyeball recedes in the orbit, it may be to the amount of half a millimetre. He sums up his results in the following words:—

"We have satisfied ourselves, that both the external vessels of the eye, and the intra-ocular and the retro-ocular vessels are distended by increased expiratory pressure. We have seen that the eyelids in closing limit or entirely remove this distention, partly by pressure, partly perhaps by a certain associated action. We have observed, that on each increased expiratory effort the eyelids are closed, or that at least a tendency to closure exists. The inference therefore readily suggests itself, that through and by the closure of the eyelids the injurious effect of congestion of blood is warded off.

"The weak point of the demonstration consists in this, that little has been shown of essential injury from want of support. However the external vessels may, under such circumstances, burst, the influence on the internal (undoubtedly placed in an abnormal condition) may on designed observation in the normal state be still more evident, and with respect to the retro-ocular it has already become probable that where the ordinary support is wanting, the distention would gradually attain a higher degree and cause disturbance: varices are met with as the cause of exophthalmos, and what bears still more directly upon our inquiry, at the last Ophthalmological Congress Dr Gunning communicated a case of exophthalmos in consequence of whooping-cough, which in his opinion must depend upon the rupture of vessels. I find that Mackenzie, in his *Practical Treatise on the Diseases of the Eye*, London, 1854, p. 309, gives a similar case."

A translation, *in extenso*, of Prof. Donders' essay will be found in Dr Beale's *Archives of Medicine* for October, 1870.

3. As in some measure relating to the subject of Professor Donders' Essay, I may refer to a paper by E. H. Sesemann, *On the orbital veins and their connexion with the veins on the surface of the head*, of which an abstract is given in the Northern Archives of Medicine (*Nordiskt Medicinskt Arkiv*), Vol. 2, Part 1, from which I take the following: "Since Walter's time the veins of the face have not been specially investigated and described. The author, who founds his statements entirely upon his own preparations, has in this work brought forward some new views respecting the veins of the eye and face. He describes a venous plexus, enveloping the parotid duct. He considers that the vena ophthalmica superior serves not merely to carry off the blood from the other veins in the orbital cavity, but at the same time to form an efferent canal, a so-called emissarium Santorini, for the sinus cavernosus. Thrombus in the sinus is therefore not sufficient to produce disturbances in the distribution of blood within the eye, to this end thrombi in the vena ophthalmica superior, or in the vena facialis anterior would also be required. The vena ophthalmica inferior likewise pours blood either into the sinus cavernosus or into the vena ophthalmica meningea. The vena ophthalmica superior has not any valves and forms a construction at its opening into the sinus cavernosus. The vena ophthalmica inferior may have a valve, but only in the rare cases in which it opens into the vena ophthalmica superior, and then at the point of junction. The vena centralis gives off strong anastomoses to the vena ophthalmica superior, it may also empty itself into the vena ophthalmica inferior, and it sometimes forms a plexus around the sheath of the optic nerve."

4. The 2nd and 3rd numbers of the 5th volume of the Transactions of the Medical Society of Upsala *Upsala Läkareförenings Förhandlingar*) contain papers by J. A. Waldenström and Aug. Almén upon a remarkable change produced in the urine by the external use of carbolic acid as a disinfectant whether applied upon poultices, or injected, in solution in oil, into abscesses. The change described ceased when the use of the acid was discontinued, and returned when it was resumed. It was observed in three cases in the University Hospital. The urine had a very dark, almost black, tarlike appearance, and an acid reaction, it contained albumen in small quantity, but no blood. On adding sulphuric acid to the urine until a highly acid reaction was produced, and distilling over about one third of the fluid, the distillate smelled strongly of carbolic acid, and gave beautiful and indubitable reactions of the same. Both writers consider that this change in the urine indicates that the external use of carbolic acid might give rise to dangerous nephritis, but Professor Almén adds his conviction that it is not therefore necessary to abstain from a judicious employment of it, and as its antiseptic powers are so great he advises that it should be used in smaller quantity and in a less concentrated form than is often done. Its good effects seem to depend less on the quantity of the acid than on its coming into general contact with the pus. He recommends

an oily solution for poulticing, but a watery solution for injection, and he advises that during the employment of the acid the urine should from time to time be tested for albumen, even though it should present no abnormal appearance.

To the foregoing it may be added that in another case mentioned by Hr. Waldenström, the *internal* use of carbolic acid produced in a syphilitic patient a transitory albuminous condition of the urine. He believes that the change of colour in the secretion, which, according to him, occurs only under the external use of the acid, is due to the presence of some unknown oxidation products of the latter.

[Other writers (Bardeleben, Lister, Wallace, &c.) have described a similar discoloration of the urine from the external use of carbolic acid; but, so far as I am aware, albumen was not found in the excretion in their cases. See *Brit. Med. Journal*, April 2 and 30, 1870; *Edin. Med. Journal*, May 1870; and Dr Walter Smith's Report, *Dub. Quart. Journal*, August 1870.]

5. Dr Olof Hammarsten of Upsala contributes to the *Northern Archives of Medicine*, already quoted, Vol. II. Part 1, a long paper upon the reaction of bile on pepton.

"Albumen, digested in the gastric juice, contains at least two bodies: *pepton* and *syntonin* (Meissner's parapepton), of which we consider the former as really dissolved, the latter, on the contrary, with Brücke, as only finely divided and greatly swollen in the acid of the gastric juice. The pepton is, so far as we know, the final product of the action of the gastric juice, it is ready digested albumen, having a low endosmotic equivalent, or, what is the same thing, a great diffusive power, and is thus, so far as we can understand, ready to be at any time absorbed. The syntonin, on the other hand, is not ready digested, it has a high endosmotic equivalent, a low diffusive power, and we may, considering its condition as to solubility, ask whether it can at all pass from the acid contents of the stomach or intestine into the alkaline blood. The question now remains, how these two dissimilar albuminous bodies behave with the bile, whether they are both precipitated by it or not?"

The author shows, from the writings of Kühne, (*Lehrbuch der physiologischen Chemie*, Leipzig 1868,) and others, that the opinion at present generally held is, that both the syntonin and the pepton are precipitated by the bile, that bile is capable of throwing down from an acid solution also the pure pepton. But believing that the subject deserved further investigation, he instituted three series of experiments, bearing, however, only on the question whether the pepton, as a rule, is precipitated, and can be completely precipitated by the bile. The results of his experiments answered this question in the negative, he found that the greater part of the pepton remains in solution and is therefore not thrown down by the bile, and he believes that in the majority of cases during life, in natural digestion, the bile does not in the intestine completely precipitate the pepton.

The author argues that this imperfect precipitation of the pepton, and the re-solution of the latter when acid reaction is present, are of much importance in the process of digestion, and help to explain

the effect of the intervention of the bile between the ventricular and pancreatic digestion. But, on the other hand, he adds, we may perhaps still have long to wait before we obtain a clear insight into the physiological signification of this intervention of the bile, and he agrees with Kühne, "that a wide, important, and fertile field, here awaits the next investigations."

6. Among the "Shorter Communications" in the same number of the *Archives*, is a paper by Professor Axel Key and Gustaf Retzius, *Lic. Med.*, "On the membranes of the brain and spinal cord, with special reference to the serous spaces and lymph-passages, with their connections."

The animals experimented on by the authors were, living or recently killed dogs, rabbits, and sheep; in addition they used a large number of human bodies.

The method employed was the injection of coloured fluids, which by gentle constant pressure, seldom amounting to 60 mm. of mercury, in a shorter or longer time were driven into the spaces under the dura mater and the arachnoid, either into these spaces separately, or simultaneously into both. Most frequently the injection has been effected from the membranes of the spinal cord and with the skull unopened, so as to obtain for the brain as clear and unimpeachable results as possible. The injection-fluids have generally consisted of Richardson's blue with glycerin, but without alcohol, as well as of a thin solution of gelatine in which fine cinnabar is suspended.

Did space permit I should gladly give the details of the authors' results. In the contrary case, I content myself with quoting the following summary, with which they conclude their interesting communication:

"We believe therefore that by these investigations we have demonstrated, that an uninterrupted connection exists between the subarachnoideal spaces[1] in the spinal cord and the brain, the lymph-canals around the vessels in the meninges and the epicerebral space, the perivascular spaces in the brain, the ventricles of the latter and the central canal of the spinal cord, the investing space around the peripheral nerves proceeding from the brain and spinal cord, and serous sheaths and spaces in the higher organs of sense; that these serous spaces of the nervous system at least partly stand in immediate connection with the other lymph-systems of the body, but that at the same time the serous fluid, both from the subarachnoideal spaces and from the subdural space, must for the most part have its efflux through the veins, and that in this the Pacchionian granulations play an extremely peculiar and important part, although the resorption through the veins in a large measure takes place also independently of them."

In the second part of the volume the authors remark further upon the transition of the fluids from the above-mentioned serous spaces into the veins through the intervention of the Pacchionian

[1] To prevent misconception we call the spaces under the arachnoid the *subarachnoideal spaces*, and the space under the dura mater, that is between the latter membrane and the arachnoid, the *subdural space* = the arachnoideal space of other writers.

granulations, and they proceed to say that "at the side of the longitudinal sinus we find in the dura mater large, hitherto unobserved, venous cavities or spaces, which in general stand nearly at right angles to the sinus, and are of irregular form. They are often connected with one another by canals of varying calibre, running more or less parallel to the sinus. Through a narrow, usually rounded, opening these cavities communicate with the sinus; on the opposite side they are connected with the other comparatively fine veins of the dura mater, which take their origin from, or open into them. In these venous cavities, in the dura mater, we have, both in adult human subjects and in new-born infants, and in the dogs and sheep we examined, constantly found extremely numerous Pacchionian granulations; often the cavities, particularly in elderly individuals, are completely filled with them. All the granulations, which usually occur so numerously near the sides of the longitudinal sinus, have either penetrated, or are about to penetrate, into these spaces; all the granulations which completely perforate the dura mater, stand with their inner parts in these spaces. These venous spaces at the sides of the sinus, with the Pacchionian granulations found in them, seem therefore to have their peculiar importance as absorbent organs, and it is precisely through their intervention, that the veins in the neighbourhood of the sinus are filled, even before the injected fluids have penetrated into the sinus. But not merely in the neighbourhood of the longitudinal sinus, but, in general, wherever Pacchionian granulations are met with, the latter penetrate into the veins and the venous sinuses, or into sinuous spaces in their vicinity."

7. To the Second Part of the same Volume, Dr Christian Lovén contributes a "Preliminary Communication" on the "Lymph passages in the Mucous Membrane of the Stomach."

"It is," he observes, "undoubtedly remarkable, that while we have of late, through a number of investigations, chiefly those of Teichmann, His, and Frey, arrived at a very complete knowledge of the lymph and chyle passages in the intestinal mucous membrane, the examination of the otherwise so similarly constructed mucous membrane of the stomach should have afforded such comparatively scanty results."

The general inference drawn by the author from his own researches is, "*that the lymph passages in the mucous membrane of the stomach have their commencement in a system of communicating openings or crevices in the connective tissue immediately surrounding the glands.* This system of cavities, which in an empty condition, and when the intervening connecting substance is swollen by the action of certain reagents, as acids, &c., exhibit themselves in stellated anastomosing 'connective tissue bodies' (Virchow), empty themselves directly into the larger reservoirs situated in the middle in the interstices between the glands, which we have learned to recognise as interglandular lymph sinuses, and which descend to the deepest layers of the mucous membrane, where they open into the already described subglandular and submucous networks of lymph-canals; first from the last named do the proper *lymph vessels*, furnished with valves and proper walls, take their origin."

Journal of Anatomy and Physiology.

NOTES ON SOME VARIATIONS OF THE PECTORALIS MAJOR, WITH ITS ASSOCIATE MUSCLES seen during Sessions 1868—69, 69—70, at King's College, London. By J. B. PERRIN, *Demonstrator of Anatomy, King's College, London.* (Pl. VII. VIII. IX. & X.)

THE *Pectoralis major* is liable to many slight transmutations; and it has frequently impinging on it, or intimately associated with it, other muscles which present several grades of development.

1. The simplest form of deviation from the average arrangement of the muscle, is that in which a part, or the whole, of the great pectoral is blended at its distal or humeral attachment with that of the deltoid.

2. The clavicular fibres of the pectoral may be continuous throughout their entire extent with those of the deltoid, and separated by a distinct and variable-sized interval from the sterno-costal fibres of the pectoral.

3. The pectoralis major and deltoid muscles may be directly continuous; no areolar interval marking the line of separation between them. When such is the case, the cephalic vein, which usually lies in the interval between the clavicular fibres of the deltoid and pectoral, is either aborted, or perforates the muscular fibres to reach the axillary vein.

4. The *portio attollens*, or clavicular-pectoral, has the same attitude towards the sterno-costal fibres as it has towards the deltoid. It may be directly continuous with them, and it may be partially or entirely separated. In the latter conditions the areolar interval varies considerably, from a mere line to an inch in width.

5. Those fibres of origin which spring from the seventh rib, and from the aponeurosis of the external oblique muscle of the abdomen, may also be divided by an areolar septum from the sterno-costal fibres. Various modifications and combinations of these slip-like differentiations exist. Rarely, however, does the muscle present such a divided arrangement as that in which all the prementioned areolar spaces are persistent. I have seen the pectoral so split up as to give it a stellate appearance, composed as it were of several radii which united at their common humeral attachment.

6. Sometimes the pectoral muscle has associated with its inferior fibres a partially separated slip, which arises from the abdominal aponeurosis, soon uniting with the pectoralis major ...generally opposite the axillary space. This seems to be an aborted form of the epigastric slip, and also closely simulates the proximal portion of an imperfect form of the chondro-epitrochlearis muscle. It is difficult to decide as to whether it belongs to the former or to the latter.

7. I have met with one instance in which a portion of the inferior fibres of the pectoralis major gradually detached themselves from the parent mass, constituting a distinct muscular slip, which passed obliquely upwards to be inserted into the coracoid process, superficial to the coraco-brachialis muscle and coracoid factor of the biceps, and external to the pectoralis minor. This was present, only, on the left side. This is a good specimen of Wood's *chondro-coracoid.*

8. In another subject I found the clavicular portion of the great pectoral divided into two slips, about an inch and a quarter from its distal attachment. The external one, mainly muscular, passed behind the long head of the biceps, and behind a capsular tendon of origin which was also present, to be inserted into the outer bicipital ridge. The internal portion was distributed after the usual manner of the undivided fibres. In one subject the clavicular portion of the deltoid gave a detached slip from its external border to join the clavicular portion of the pectoralis major.

9. Besides these simple forms of variation the great pectoral has occasionally connected with it other muscles of a more complex character. These are:

1st. Epigastric slips.

2ndly. Muscular, or, musculo-tendinous, or, entirely tendinous slips from the latissimus dorsi.

3rdly. Varieties of the chondro-epitrochlearis muscle.

10. 1st. *Epigastric slips.* (Fig. 1. a'. a''. a'''.) These are moderately frequent, and present considerable differences in number, size and arrangement. They may divided into simple and compound.

a. Simple varieties. Both the simple and compound slips are most commonly situated below, and parallel with, the lower border of the pectoralis major. Sometimes they are continuous with that muscle at their origin; oftentimes they are separated from it by an areolar interval. They may arise from the seventh rib; from the sixth and seventh; from the seventh rib, and the aponeurosis of the external oblique; and, from the latter only. They are inserted, most frequently, with the *pectoralis major* tendon, just before its attachment to the humerus. Sometimes they are inserted into the fascia, covering the coracobrachialis, constituting the chondro-coracoid of Wood. Again, they may fall short of either of the preceding attachments, terminating in an expansion which is gradually lost in the cellular tissue of the axillary space. In one instance only have I found an epigastric slip joining the tendon of the *latissimus dorsi.*

b. Compound varieties. 1. These consist of two distinct bands of muscular fibres, which run parallel to each other, and to the lower border of the pectoralis major (sometimes also to the minor, resembling a differentiated continuation of the latter muscle), terminating separately, or uniting just before their insertion.

2. Of three slips. This is a rare form of epigastric, and may be partial or complete.

3. Either of the preceding varieties, simple or compound, may be complicated by the addition of a slip from the *latissimus dorsi* (Achselbogen).

In twenty-nine subjects, dissected at King's College Anatomical Rooms during the winter session 1868—69, I found epigastric slips in six. Four occurred in males; in three on both sides, and in one on the left side: two in females; in one on the left, and in the other on both sides.

In the same number of subjects dissected during the session 1869—70, I found them in five. Four occurred in male subjects; in two on both sides, in one on the right side, and in one on the left side: in the female subject, the epigastric was present on the right side only. Thus, in fifty-eight subjects, epigastric slips occurred in eleven; in eight males and three females; in five males on both sides; in two on the left, and in one on the right side; in the three females, in one on the right, one on the left, and in one on both sides.

In the 1868—69 series, four arose from the sixth rib, one from the seventh rib, and one from sixth and seventh ribs: three out of the six were inserted into the pectoralis major tendon: one joined the latissimus dorsi tendon; and two were inserted into the coraco-brachialis fascia.

In the 1869—70 series, three were attached to the sixth rib, and one to the aponeurosis of the external oblique muscle of the abdomen; one was peculiar and will be presently described; three were inserted into pectoralis major tendon, as in the 1868—69 series, and one into the coraco-brachialis fascia. In one of the 1868—69 series (a muscular male), the pectoralis major presented its usual average size and arrangement. Arising below it, and partly overlapped by its inferior fibres, were two broad muscular epigastric slips; one from the fifth and sixth ribs; the other, inferior and larger, from the seventh rib. Both slips passed, in a direction obliquely upwards and outwards, parallel with the lower border of the pectoralis major muscle, across the axillary cavity, narrowing somewhat in their course. At the middle the inferior of the two gave off a small muscular slip to the upper one. About two inches from their distal extremity they joined together, and were soon afterwards joined by an unusually large and entirely muscular slip from the latissimus dorsi. Immediately succeeding the latter addition the entire muscle, thus constituted, terminated in a small, short, and flattened tendon, which was inserted into the outer bicipital ridge of the humerus along with the pectoralis major tendon, the lower and posterior aspect of which it joined.

In one of the 1869—70 series (a very muscular male) was another peculiar form of epigastric slip: at least, it is more

closely allied to the epigastric slips than to anything else. It might, however, be called an intermediate pectoral muscle. It consisted of a long, thin, and somewhat tapering band of muscular fibres, which was attached proximally to the fourth rib, close to its cartilage. It crossed the superficial aspect of the pectoralis minor obliquely, separated from it and also from the major by a fascial investment. It was inserted into the upper border of the pectoralis major tendon, close to the bicipital ridge, and by an expansion prolonged upwards with the capsular ligament of the shoulder-joint. This was present on the left side only.

I met with a larger specimen of the same kind in a muscular male subject during the present session (1870—71), occurring also on the left side. It presented no difference from the preceding except in its large size, measuring an inch and a quarter across. This slip, as well as a part of the serratus magnus, was supplied by a twig from the 4th intercostal nerve. I could not trace any nerve from either of the thoracic branches given to it. The accompanying sketch (Fig. 3 *a*) shows the analogue of this muscle in the Squirrel. I have a sketch of a similar muscle which I found in the Alligator. If attention is directed to this point particularly, I have no doubt that it will be more frequently found, as it is very liable to be divided along with the great pectoral, and thus overlooked. These are the only two instances which I have met with in nearly one hundred subjects.

2ndly. *Slips from the Latissimus dorsi* (*Achselbogen*).

These so-called aborted varieties of the dorso-epitrochlearis muscle are pretty constant in their size and arrangement, and do not present any decided variation, except in a few particular instances. (Fig. 2 *h″*.)

In twenty-nine subjects (session 1868—69) the Achselbogen occurred in seven subjects, five males and two females. In four males it was present on both sides, and in one on the left side only. In one female it was present on both sides, and in the other on the left side only.

In twenty-nine subjects (session 1869—70) I found it only three times in as many muscular male subjects, on both sides in all. Its frequency is seen to coincide very nearly with that

of the epigastric slip. In these anomalous muscles, as well as in others, a very large average must be allowed before trustworthy evidence can be accorded as to their relative frequency. It is quite possible that not a single specimen should be found in fifty subjects in one dissecting room, while in another as many as a dozen might be present in a corresponding number of subjects. These slips seem to be aborted specimens of the panniculus.

3rdly. *Chondro-epitrochlearis.* Several varieties of this peculiar muscle are met with in the human subject; *first* a simple musculo-tendinous band extending from the ribs to the epitrochlea. Fig. 4 $a'a'$, represents a simple yet complete form of the muscle. It arises from the seventh rib, in conjunction with the inferior fibres of the pectoralis major. It may also be connected with the aponeurosis of the external oblique muscle, in the same manner as the epigastric slips. It is closely associated with the lower fibres of the great pectoral, and continuous with them as far as the centre of the axillary space. The muscular fibres there separate from the pectoral, as a condensed and distinct muscular band, which at first gradually arches downwards, and then traverses more or less vertically the superficial aspect of the arm as a moderately slender tendon, anterior to the biceps muscle, and almost parallel with the internal intermuscular septum, and terminates at the internal condyle of the humerus.

Fig. 5 $a'a'$, shews a compound variety of this muscle. The muscular part arises and is disposed as in the preceding; being, however, somewhat (½ inch) wider and extending further on to the anterior aspect of the arm, terminating opposite the middle of it in a moderately strong tendon, which descends from the capsular ligament of the shoulder-joint in front of the long tendon of the biceps. The distal insertion of the muscle is similar to that of the preceding.

Fig. 6 aa'', shews a very eccentric form of this muscle, which closely simulates the extensor plicæ alaris of the bird. It is similar in its general arrangement to that of Fig. 4, but differs from it in having an additional wedge-shaped muscular contribution from the middle of the coraco-brachialis, about an inch long and 2½ lines wide, which becomes tendinous

immediately prior to its attachment to the brachial tendon of the chondro-epitrochlearis.

There is yet a modification of the capsular portion of the chondro-epitrochlearis which consists of the capsular band of fibres only, and which joins the short head of the biceps muscle; it is about two inches long.

Fig. 7 *aaa*, shews the dorso-epitrochlear muscle. It is like the chondro-capsular form, except that the muscular part is derived from the *latissimus dorsi* instead of the *pectoralis major*. There is another variety, and I am not certain whether it belongs to the chondro-epitrochlear series or not. It is tendinous throughout its entire extent, arising from the glenoid head of the biceps immediately after it emerges from the capsular ligament. It runs parallel with the outer border of the biceps muscle, terminating in a fascial expansion immediately below the elbow-joint, on its radial side, and superficial to the deep fascia. Varieties similar to the preceding, with some few exceptions, have been described by Professor Wood in his papers on Variations in Human Myology, published in the *Royal Society's Proceedings*. Other observers have recorded these anomalies, *e.g.* Dr Macalister, Professor Turner, &c. The object of this paper is not to give a comprehensive view of what has been done by other observers, but merely to give a brief outline of the irregularities of this class which I have found, during two winter sessions, in the dissecting-room at King's College, London.

I cannot enter, in this paper, into a discussion upon the homologies and analogies of these peculiar forms of muscles—namely, the epigastric and chondro-epitrochlear. I will simply offer a few suggestions as to their actual source. If we examine the bird's wing we shall find both the epigastric slip, and a muscle to all intents analogous to the chondro-epitrochlearis. In the common barn-owl there is a muscle which arises from the fifth and sixth ribs. It passes obliquely upwards and outwards, terminating in from three to five small and short digitations, partly muscular, and partly tendinous, which are inserted into the barbs of as many feathers, situated upon the upper and inner aspect of the wing. In the cormorant this slip is very large and well developed. It is

then attached by its proximal extremity to the sixth and seventh ribs. Professor Owen describes this as a portion of the serratus magnus. It has, however, nothing whatever to do with that muscle. It is this muscle (I believe) which finds its homologue in the epigastric slip of the human subject.

As regards the chondro-epitrochlearis, and its dorsal representative, the former seems to me to be the relic of the extensor plicæ of the bird, and the latter a representative of the dorso-humeral portion of the panniculus of quadrupeds. In the wood-pigeon and other birds, the muscular slip which is detached from the biceps (in some birds arising by a separate fascia from the front of the biceps) closely resembles the wedge-shape muscular band from the coraco-brachialis in Fig. 6. In the *wood-pigeon* it joins the extensor plicæ alaris, just as in this case the coraco-brachial slip joins the chondro-epitrochlearis. All these are more complete forms of the "portion ventrale" of Cuvier's and Laurillard's plates; of the "costo-humeral" of Professor Huxley; and the chondro-epitrochlear of Duvernoy (Wood, *on Variations in Human Myology*, 1868).

ON A RUDIMENT OF THE DORSAL PORTION OF THE PANNICULUS CARNOSUS, SUPERFICIAL TO THE TRAPEZIUS. By J. B. PERRIN, *Demonstrator of Anatomy at King's College, London.*

IN the last No. of the *Journal of Anatomy and Physiology*, Professor Turner describes a dorsal portion of the *Panniculus Carnosus*, superficial to the *Trapezius* muscle. During the winter session of 1868 I met with a corresponding muscle in a muscular male subject dissected in the Anatomical Rooms at King's College. It was attached below by musculo-tendinous fibres to the spines of the 8th and 9th dorsal vertebræ on the right side. From this origin it passed almost vertically upwards, and nearly parallel with the dorsal vertebræ, as high as the first, where it became tendinous and arched downwards and inwards to the spine of the second. From the convexity of the tendinous arch two or three slips radiated upwards and outwards, and were lost in the subjacent fascia. The muscle was somewhat fusiform in shape, and its muscular fibres occupied the greater portion of the muscle. It was covered by the skin and superficial fascia, and rested on the fascia covering the *trapezius*. The origin of the *trapezius* was normal. The muscle I have described, except in being longer and wider, arising as low down as the 8th dorsal spine, is identical with that described by Professor Turner. In my note-book I had entered the muscle under the name of *dorsofascialis*, a rudiment of the *panniculus carnosus.*

ON THE ANATOMY AND PHYSIOLOGY OF THE SO-CALLED 'SALIVARY GLANDS' OF THE COMMON COCKROACH—PERIPLANETA ORIENTALIS. By W. AINSLIE HOLLIS, M.B., *Cantab.* (Pl. XI.)

THE labium of this insect consists of seven parts, six of which are arranged laterally in pairs. Anteriorly is a single fleshy organ, which, from its form, its position in the oral cavity, and its probable functions, may be called the 'lingua' (fig. 1, *c*). Inferior to, and somewhat behind the lingua, are the 'sublingual palpi' (*ee*), two flapper-like organs, consisting each of three joints, and affixed at their base to the 'labium' proper (*g*). This last organ consists of two thin chitinous plates, separated from each other by a longitudinal division, and united at their bases to the lingua. Lastly, there are two 'labial palpi' (*ff*), one of which is placed on each side of the labium. Over the dorsum and tip the surface of the lingua is covered with short papilliform bodies, which probably assist the passage of the food in the process of deglutition. Towards the root are two or three rows of tubercles, arranged after the manner of the circumvallate papillæ of the human tongue. Near this point there is a longitudinal mesial depression terminating in a cul-de-sac. The posterior or under surface of the lingua is deeply sulcated along the mesial line (fig. 4, *b*), the groove thus formed terminates at the base of the organ in an irregularly-shaped opening (*c*) surrounded by a delicate framework of chitine. This opening resembles that of the human larynx in shape, and is supplied inferiorly with an appendage similar to the epiglottis, which possibly acts as a valve. From its position, external to the true oral cavity, the orifice in question is freely exposed to the air when the lateral divisions of the labium are separated from each other and the valve is open. When traced upwards this aperture is found to communicate with an infundibular cavity, connected at its further extremity with a centrally placed tubule. This tubule, partly from its position in the oral orifice, and partly from its relations to some organs to be described, has been called the 'salivary duct.' If, however,

it be carefully dissected from its surrounding connections as it passes upwards along with the cords uniting the first (sub-œsophageal) and second post-oral ganglia, it will be found, under the microscope, as has been observed by others, to possess the well-marked anatomical peculiarities of an ordinary tracheal vessel. Passing abruptly backwards for about 0·07″, in close proximity to the ganglionic cords and below them and the œsophagus, it gives off from its upper surface a branch (*trachea ramalis*) (fig. 1, *b*) about one-third the calibre of the main vessel; and the two continue their course parallel to each other for about 0·03″. Near to this point a bifurcation takes place in both, the lower and larger vessel (*trachea saccularis*) (fig. 1, *a*) dividing somewhat anteriorly to the other. If the course of the two branches of the trachea saccularis is traced, they are found to pass round the nervous cords, one on either side, until they are about 0·25″ in length, when each suddenly expands into a pyriform sacculus—the so-called 'salivary bladder'—(0·35″ in long diameter). The corrugated inner membrane of the trachea is gradually lost near the orifice of the sac, while the outer membranes of the vessel are expanded over its surface, and form an exceeding delicate hyaline covering with an internal fibro-cellular coat. I have never found a liquid in these sacculi, nor have I seen them in other than a collapsed state. During its passage around the nervous cords each of the branches of the saccular trachea receives a large nerve, which passes along between the outer coats of the vessel and sends off branches to the sacculi and the 'dendritic' bodies to be hereafter described.

The ramal trachea after its bifurcation pursues a similar course to the saccular, the branches running in juxtaposition with those of the saccular trachea for about 0·15″. They then commence to divide dichotomously, and continue so to do until the ultimate leaflets of the dendritic bodies are attained. The ramal trachea in its whole length and the saccular in certain parts are invested with a hyaline membrane, between which and the proper tunics of the tracheæ is a space filled with nucleated elliptic bodies (about the size of lymph corpuscles) and a granular yellowish-white material. This hyaline and corpuscular investment is of varying thickness, but is far more developed upon the ramal than upon the saccular tracheæ. En-

veloping the finest branches of the ramal tracheæ it accompanies them throughout their somewhat convoluted course until they have arrived at their ultimate ramifications, where it appears to form the chief substance of the cordate leaflets of the dendritic bodies (fig. 2) into which the ramal tracheæ expand.

The two delicate arborescent bodies thus formed, designated in modern hand-books[1] the 'salivary glands,' are, in the adult insect, about 0·45″ in long diameter, or upwards of one-third the entire length of the animal. The comparative size is much less in the young insect. By their upper and inner surface they are applied to the under part of the œsophagus, and are retained in their position to a great extent by the ramifications of the œsophageal tracheæ over their surface. These œsophageal tracheæ pursue a course distinct from the ramal trachea, and nowhere enter the substance of the bodies in question. Between the two secondary branches of the ramal trachea, on either side, is placed the corresponding sacculus of the saccular trachea. A delicate net-work of fibres connects the sacs with the leaflets and the latter peripherally with one another. These fibres were at first supposed to be bands of connective tissue, but may possibly be nerve fibres. They anastomose frequently with each other at short intervals, and contain in their interior occasional deposits of pigment matter with oval and stellate nuclei and granules. The fibres can be traced passing beneath the external hyaline coat common to the sacculi and leaflets, and they then seem to be assimilated to the inner coats of the sacculi and leaflets.

What can be the functions of these dendritic bodies? We are forced to set aside the hypothesis that they are in any way connected with the production of saliva for the following reasons:—

1. The position of the orifice of the common trachea upon the posterior and under surface of the lingua, and therefore external to the true oral cavity.

2. The so-called 'common duct' presents simply the appearance of an ordinary trachea, both as regards the spirally corrugated arrangement of its internal coat, which of itself

[1] Cf. Rolleston, *Forms of Animal Life*, p. 200 (1870). Huxley, *An Introduction to the Classification of Animals*, p. 57 (1869), *et complures alios.*

points to its function as the transmitter of a gaseous fluid[1] rather than a liquid, and the nucleated appearance of its outer coats.

3. It can be readily shewn that all the branches of the ramal and saccular tracheæ open into this common vessel, wherefore any fluid in one part will probably find its way into any other. Thus, if we shew that the 'common duct' transmits a gaseous fluid (such as air), all portions of this system will probably contain that fluid.

4. If the so-called 'salivary receptacles' were intended for the retention of a liquid secretion, we should expect to see some of this fluid occasionally within them. In upwards of thirty dissections made by me, these sacs have, upon opening the thorax, always been found collapsed and apparently empty.

5. That the 'common duct' together with the 'receptacles' contains an elastic fluid is proved by the fact that it is possible to inject them by using the method employed to inject the air-tubes of the body generally: that is, by placing the insect immersed in a suitable liquid beneath the exhausting receiver of an air-pump.

6. The collapsed condition of the sacculi is such as would of necessity result upon opening the thorax, supposing these delicate bodies contained air previously, or at all events a gaseous fluid freely communicating with the air.

7. We have no reason for supposing this fluid is other than air.

I have thus attempted to shew how unfitted these sacculi and vessels are for the retention and transmission of any liquid secretion, and how well adapted they are, on the other hand, for the function of aëration. If the dendritic bodies, therefore, are of a glandular nature, they belong to the category of ductless glands.

[1] Excluding the so-called 'salivary ducts,' I can find no instance in insects of a vessel with a spirally corrugated inner membrane, formed for the transmission of a liquid secretion. The 'false tracheæ' observable in the proboscides of some insects differ both anatomically and physiologically from tracheæ. (Cf. Lowne, *Anatomy and Physiology of the Blow-Fly*, 1870.)

DESCRIPTION OF THE FIGURES.

FIG. I. Labium of the cockroach, shewing the origin of the common trachea.

a. One division of the bifurcating saccular trachea;
b. Ditto of the ramal trachea;
c. Anterior surface (dorsum) of lingua; *d* is placed over the orifice of the common trachea;
ee. Sublingual palpi;
ff. Labial palpi;
g. One of the divisions of the true bifid labium.

FIG. II. A portion of the dendritic bodies, shewing the ramifications of the ramal trachea and their expansion into leaflets. At '*a*' the investment is seen to be continuous with the substance of the leaflet. (This specimen had been steeped in glycerine for upwards of six years, and did not shew the dichotomous divisions of the trachea distinctly. The network of (nerve?) fibres is however shewn at '*b*, *b*.')

FIG. III. A portion of the common trachea near the oral orifice. The spiral corrugations of its internal coat are apparent, and at the circumference (*a*, *a*) the hyaline outer coats can also be seen.

FIG. IV. Under (or posterior) surface of the lingua; *a*, the common trachea; *b*, the sulcus; *c*, the orifice of the common trachea; the softer membranes appear to be attached to a firm chitinous framework.

OBSERVATIONS ON PHYSIOLOGICAL CHEMISTRY.

By JAMES BLAKE, M.D., F.R.C.S., *San Francisco, California.*

THE object of the following observations is to consider the bearing of some recent chemical discoveries on physiology, and to call the attention, not only of physiologists, but of chemists, to certain facts which I think show that in living substances we possess reagents which may be made available for investigating the chemical properties of other compounds. Before, however, entering on the direct subject of my communication, I would offer a few remarks on the method that is being followed in studying the chemistry of living compounds. Owing to the complex nature of these investigations it is most important that a right method should be pursued in conducting them. Here, as in every other branch of scientific research, the only true path to pursue is by gradually proceeding from the more simple to the more complex, and thus arriving by slow but certain steps from the known to the unknown; and yet in no other department of scientific research is this simple rule so completely ignored as in physiology. How large a share of physiological investigation is devoted, for instance, to researches on the nervous element, one of the most complex of living substances, and one whose properties are probably the most dissimilar from those substances with the chemistry of which we are best acquainted. After making so unfortunate a selection as regards the field for investigation, physiologists have, I believe, been equally at fault in the choice of their reagents, these having been selected from amongst substances with the chemical and physical properties of which we are most ignorant, fully justifying the assertion of Drs Brown and Fraser at the beginning of their interesting memoir "On the connection between chemical constitution and physiological action," &c. (*Journ. of Anat. and Physiol.* Vol. II.), where they state, "Unfortunately we know next to nothing of the constitution of the majority of those substances, the physiological action of which has been most carefully investigated." What would be thought of a chemist, who,

about to enter on the investigation of a new class of compounds, should make such an assertion regarding the reagents he was about to employ. The analogy between such investigations and those pursued by the alchemists in the middle ages becomes apparent if we substitute the term *philosopher's stone* for *remedies*, and *transmutations* for *convulsions, paralysis* and *death*. A fact, undoubtedly, is occasionally ascertained, which has some practical value, as was the case in the researches of the alchemists; but had the time and labour devoted to such investigations been given to a more methodical pursuit of the subject, there can be no doubt but that far greater progress would have been made, and we should not, as at present, find so large a space in physiological literature taken up in refuting the errors of previous observers. I believe it is only by selecting our reagents from amongst the more well-known inorganic compounds, and using them in the first place to investigate the least complicated reactions of living substances, that we can arrive at truths which will serve as the basis for further progress. The results that I have myself obtained by the use of these reagents proves, I think, how available they are for physiological investigations. A few experiments with these inorganic compounds suffices to show that all the salts of the same base produce analogous effects when introduced directly into the blood; and it required but a more extended series of experiments with the same substances to discover the law that their physiological action is connected with their isomorphous relations. In carrying out these researches I meet with many interesting facts, the further investigation of which I feel confident would open out a rich field for physiological research, but which I was unable to follow up on account of the great number of experiments required for even a preliminary survey of the subject I was investigating[1].

[1] Twenty-five years ago, in a paper read before the British Association for the Advancement of Science, I observed, "The fact that we now possess the means of producing well-marked and definite modifications of some of the most important physiological properties of various organs, and this too by means of reagents, the laws governing whose action we are acquainted with, places in our hands an instrument for discovery which has hitherto been wanting in physiological investigations. The enumeration of some of the effects that can be produced on the more important functions, will, I trust, suffice to lead others into this rich field of enquiry. As regards the functions of the heart, we can annihilate or increase its irritability, quicken or diminish its pulsations, render

In regard to the bearing of recent chemical investigations on physiology, I shall confine myself to calling attention to the facts that have recently been discovered in the chemistry of the hydrocarbons and ammonias, as to the importance of the form in which the elements of these compounds are arranged as determining their chemical properties. In these substances almost an infinite number of compounds can be formed by substitution, many of which, although formed by exactly the same groups of atoms, yet possess different properties. For instance, by substituting in the benzyle atom ($C_3 H_6$) two atoms of H by two of carboxyle (COOH) we have a body composed of $\begin{matrix} C_6 \\ H_4 \end{matrix}\left\{\begin{matrix} COOH \\ COOH \end{matrix}\right\}$. These elements form three distinct compounds, although there can be no doubt but that in all the three compounds the same combination of atoms exists, so that in each compound we have the group (C_6H_4) and two of the group (COOH), the differences in their properties depending on the position occupied by the carboxyle atoms in relation to the benzyle residue (C_6H_4).

The importance of the relative position occupied by the atoms in these organic compounds would tend to connect their chemistry with that of living substances, as I think there can be but little doubt that the relative position of the atoms in these bodies determines the form of the compound atom; and I have shown that in living substances the reactions that take place when they are treated with inorganic reagents, are more dependent on the isomorphous or form-element[1] of the reagents than

them regular or irregular, augment their force or render them weaker, destroy the irritability of the auricles, whilst that of the ventricles remains; keep up the circulation of the blood many minutes after every other sign of life has disappeared, and this too more actively than when respiration was being carried on; we can facilitate or arrest the passage of the blood through the pulmonary or systemic capillaries, produce important modifications in the functions of the brain; in short, the injection of inorganic substances directly into the arteries and veins enables us to modify all the more important functions of the body; and this, as before stated, by reagents, the laws of whose action we can fairly hope to discover. My reason for neglecting the closer investigation of these interesting phenomena, was a determination fully to establish the law of the analogous action of isomorphous substances. This having been accomplished, I shall now direct my researches to the elucidation of these secondary questions." (See *Report of the British Association for the Advancement of Science* for 1846.) Circumstances prevented my carrying out this intention, but I certainly am surprised that those who are more fortunately situated have not directed their attention to the subject.

[1] It will perhaps render my meaning clearer if I give an example of what I mean by the form-element in chemical compounds. If we take, for instance,

on their ordinary chemical properties. The susceptibility of living substances as reagents for the form-element of a body, will, I think, render them available for determining the constitution of isomerous organic compounds—an investigation often attended with much difficulty by ordinary chemical reagents. A striking example, showing the availability of living substances for chemical investigations, has just presented itself. Soon after the discovery of thalium, Claude Bernard made experiments by injecting some of its salts directly into the blood. On finding that the reactions produced were not analogous to those caused by the salts of potash (with which substance thalium was then supposed to be isomorphous), he arrived at the conclusion that the law of the analogous action of isomorphous substances was not true. At the time I doubted the correctness of the chemical or physiological fact. Rammelsberg has lately shown that this supposed isomorphism between potash and thalium does not exist; that thalium is probably a bivalent element, and that it forms an oxide Th_2O_3 analogous to ferric oxide. (*Chemisches Central-Blatt* No. 1, 1871.) The experiment of Claude Bernard, had he been able to interpret it correctly, pointed to this conclusion many years before the discovery of Rammelsberg; and, instead of furnishing any evidence against the law I had pointed out, offers another example of its correctness. I feel confident that carefully conducted physiological experiments will furnish chemists with the easiest means of determining the isomorphous relations of those substances whose classification in this respect have not yet been ascertained.

nitrate of silver and nitrate of soda, we find them very dissimilar in their ordinary chemical properties, so that they can rarely replace each other as reagents in chemical investigations. When, however, we mix them with living matter, they give rise to analogous reactions, and on seeking what property they possess in common that may explain this analogy, we find that they are isomorphous, or that their compounds tend to assume the same form, so that this form-element appears to be the predominant influence in their physiological reactions.

ON A PECULIAR ADDITIONAL DIGASTRIC MUSCLE.

By J. B. Perrin, *Demonstrator of Anatomy, King's College, London.*

In a moderately muscular female, æt. 70, dissected in King's College Anatomical Rooms during the present session, I found a peculiar muscle which I had not previously met with. It was a two-bellied muscle. The anterior belly (Fig. 1 A) was divided into two longitudinal portions by an imperfect areolar interval: of these two portions the inferior was attached to the greater and lesser cornua of the hyoid bone, extending as far

Fig. 1.

Refs. Fig. 1. *A*, *A'*, peculiar digastric muscle. (*a*) Internal carotid artery (cut). (*b*) External carotid. (*c*) Lingual artery. (*d*) Facial. (*e*, *f*) Hypoglossal nerve. (*g*) Hyoglossus muscle. (*h*) Digastric and stylo-hyoid muscles (out of position). (*i*) Middle constrictor. (*l*) Sterno-cleido-mastoid.

forward as the body, passing behind the hyoglossus muscle, and lying between it and the middle constrictor muscle of the pharynx, being closely associated with the latter muscle. The superior portion of the anterior belly did not extend so far forwards, but terminated in a fascial expansion over the antero-lateral aspect of the middle constrictor muscle, immediately above the preceding. The anterior belly was crossed superficially by the lingual and facial arteries, and by the continuation of the external carotid above these branches. It then emerged from between the external carotid and the occipital arteries, close to the origin of the latter, and passed in a direction obliquely upwards, outwards, and backwards. It then crossed the occipital artery, the hypoglossal nerve and the internal jugular vein, lying immediately below, and almost parallel with the posterior belly of the digastric muscle. It continued its course between the parotid gland and the sterno-cleido-mastoid muscle, crossing superficial to the latter,—a short tendon of intersection intervening between the two bellies at this point. The hinder belly was inserted into the fascia covering the occiput immediately over the external occipital protuberance, superficial to the trapezius muscle. The anterior belly was a little larger than the posterior: it was less than a quarter of an inch wide. This muscle was present on both sides. That on the right side was smaller than that on the left. Figure (1) is from a sketch of the muscle, with its relations, made from the subject by Mr. Sherwin (Artist).

In another fairly muscular female I met with a similar, but a smaller specimen. In this instance it was present on the right side only, and terminated at the great cornu of the hyoid bone. In both cases the digastric and stylo-hyoid muscles were present, and presented their normal arrangement.

In a third female subject I found another variety of this muscle. It consisted of a slender muscular band (Fig. 2 *f*), four inches long, and a line and a half wide, which arose by a strong, flat tendon, a little more than a quarter of an inch wide and half an inch long, from the mastoid process of the temporal bone, and the adjoining portion of the superior curved line of the occipital bone; situated between the sterno-cleido-mastoid muscle (*h*, *i*) (which was smaller than usual), and a large and

well developed cleido-occipital muscle (g). It crossed superficially to the sterno-cleido-mastoid, in a direction downwards,

Fig. 2.

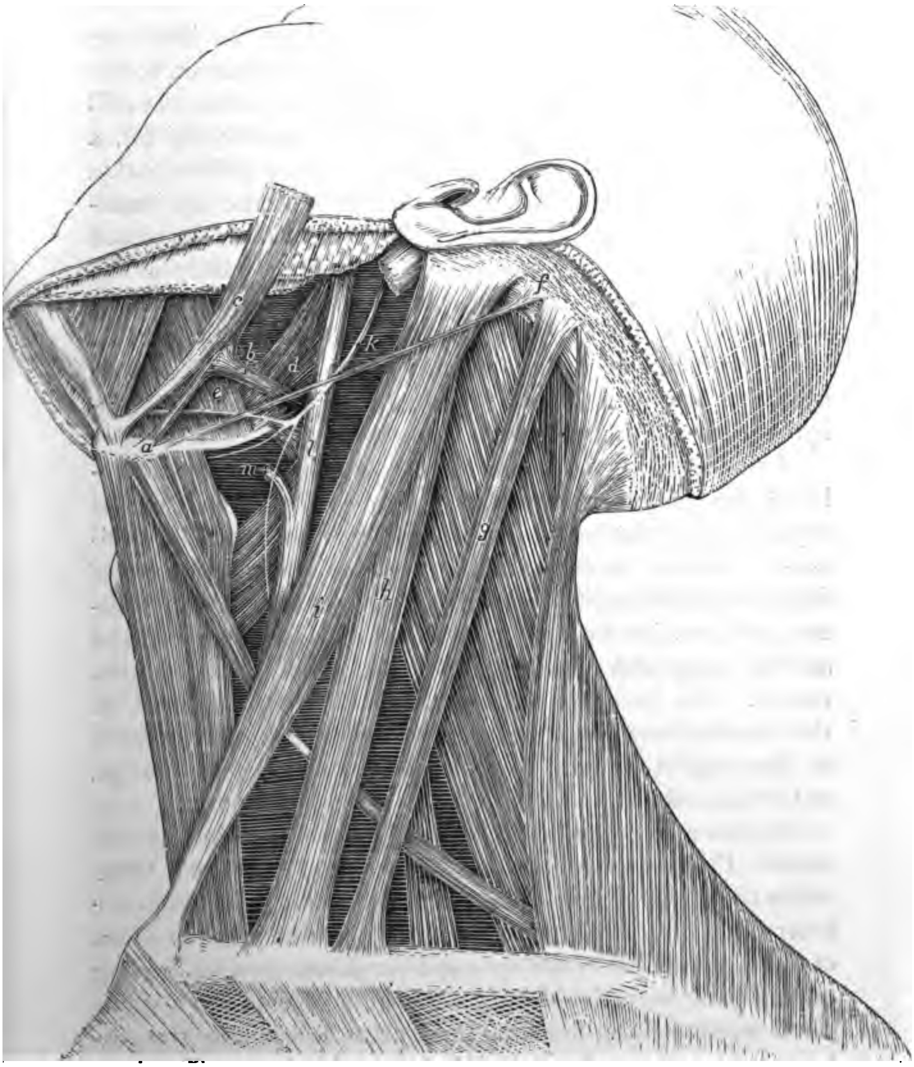


Fig. 2. Left side—female. Refs. (a) Slip from posterior belly of digastric to hyoid bone. (b) Slip from middle constrictor (d). (c) Posterior belly of digastric cut through and turned up. (e) Hyoglossus muscle. (f) Occipito-hyoid slip. (g) Cleido-occipital. (h, i) Sterno-cleido-mastoid. (k) 9th nerve. (l) Internal jugular vein. (m) Facial vein cut through.

forwards and inwards, and traversed the upper aspect of the carotid triangle lying upon the sheath of the carotid vessels, and was finally inserted into the body of the hyoid bone, immediately above the omo-hyoid muscle. This slip received, close to its insertion, an accessory muscular band (*a*), an inch long and two lines wide, which was detached from the lower border of the posterior belly of the digastric (*c*). The latter was still further complicated by the addition of a muscular slip (*b*), a little more than an inch long, which arose as a differentiation from the middle constrictor muscle of the pharynx (*d*), immediately above the great cornu of the hyoid bone. It passed almost directly forwards, and joined the posterior belly of the digastric close to the commencement of its tendon. The 9th nerve intervened between it and the great cornu of the hyoid bone. This muscle occurred on the left side only. The stylo-hyoid was wanting. On the right side the digastric and stylo-hyoid preserved their normal arrangement.

I also found a smaller and aborted specimen of the occipito-hyoid slip in a muscular male. It arose from the superior curved line of the occipital bone, posterior to the cleido-occipital, which was again present. It crossed the upper part of the sterno-cleido-mastoid superficially in an arch-shaped manner, and was inserted into the lower and back part of the auricle, along with the lower fibres of the retrahens aurem muscle. The latter muscle, as well as the attollens, were, in this instance, continuous at their origin with the posterior belly of the occipito-frontalis, diverging from it as finger-like slips, as they passed forwards and outwards towards the auricle.

Professor Wood has described and figured a variety of this muscle (*Proceedings of the Royal Society*, p. 522, No. 93, 1867) under the name of mylo-glossus. "It arose tendinous from the inner border of the lower jaw, and was inserted into the fibres of the tongue between the stylo and hyoglossus muscles, joining especially the latter." Mr Wood states that Henle saw a cylindrical muscle arising from the same place, and joining the posterior belly of the digastric (*Muskellehre*, 5, 112). The last-mentioned peculiarities are evidently analogous to the stylo-hyoid muscle of birds, closely according in origin and in insertion with that of the anterior division of the stylo-hyoid.

As regards the four varieties which I have described, I am inclined to believe that they are simply anomalous forms of the same muscle. I have met with the occipito-hyoid slip in the common seal. It arose, however, from the mastoid process of the temporal bone, immediately behind the auricle, superficial and external to the sterno-mastoid muscle. It crossed the large digastric muscle, and terminated in the hyoid bone, close to the outer fibres of the hyoglossus muscle, having the hypoglossal nerve immediately above it, and the omo-hyoid muscle below it. This muscle is figured and described in Cuvier and Laurillard's plates of the common seal as the masto-hyoid muscle. In the specimen which I dissected—and apparently from the figure of it in Cuvier's and Laurillard's work—this so-called masto-hyoid did not pass beneath, but superficial to, the hyoglossus muscle, as the inferior portion of the anterior belly of the irregularity shewn in Figure 1 A; but simulated more the 2nd and 3rd specimens described.

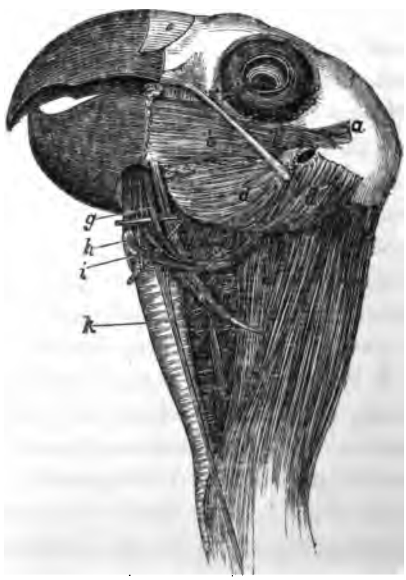
Douglas, in his *Myographia Comparata*, describes a muscle in the dog—'the inio-cerato-hyoideus'—which arises from the occiput, and is inserted into the longest process of the os-hyoides (p. 38). The same muscle is also described in Cuvier and Laurillard's work (*Myologie de la Hyène Rayée*), "Outre ces stylo-hyoïdiens il existe un petit ruban musculaire tout a fait externe qui se rend de l'apophyse mastoïdien à l'os hyoïde (il est indiqué, s.s., Pl. 131, 132)."

Professor Wood says that he has found a modification of this muscle in the cassowary, the Cape rat mole, the badger, and the hedgehog. I have examined the latter animal several times, but have failed to meet with anything simulating the predescribed muscle. I have examined the guinea pig, the Norwegian rat, the mouse (brown and white), the hare and rabbit, with similar results. All the afore-mentioned varieties find their primary homologue in the differentiated stylo-hyoid of birds. The muscle (Fig. 1 A) in the human subject, passing behind the hyoglossus, is seen to closely simulate the anterior of the three divisions of the stylo-hyoid in the bird (Fig. 3 *g*), which has also a similar arrangement, passing behind the hyoglossus, to be inserted into the basi and glosso-hyals.

The slip from the digastric muscle (*a*, Fig. 2) simulates the middle slip of the avian stylo-hyoid, and an aborted portion of

the normal mammalian stylo-hyoid; the occipito-hyoid slip in Fig. 2 resembles more that of the posterior slip of the avian stylo-hyoid—rather than its representative in Fig. 1—the anterior slip as prementioned. These anomalous forms—and others described by other observers—of the stylo-hyoid, clearly indicate its tendency to aberration, both as to its point of origin and its general arrangement. It seems to have a tendency towards a minus, rather than a plus, grade of development in the higher animals; while the reverse is the case with the digastric muscle. The stylo-hyoid wastes, or becomes aborted, in its progress from the mandible towards the occiput; while the digastric increases in size and power as it becomes directed into two muscular portions, and attains a hyoid as well as a maxillary attachment. The latter is evidently slowly, but gradually, effacing the former. The comparative infrequency of double stylo-hyoid in the human subject and its minus tendency seem to afford strong evidence in favour of such a view.

Fig. 3.



Refs. to Fig. 3. Muscles of the mandible and hyoid, &c. of the grey parrot (Psittacus erythacus).

(*d''*) Digastric (biventer maxillæ of Owen). (*f*) Stylo-hyoid, dividing into three portions: anterior (*g*), middle (*h*), and posterior (*i*).

N.B. These are supported in the figure by small bristles.

OBSERVATIONS ON THE PHYSIOLOGICAL ACTION OF THE HYDROCHLORATE OF COTARNAMIC ACID. By J. WICKHAM LEGG, M.D.

THE substance which has been called the hydrochlorate of cotarnamic acid is obtained by the action of hydrochloric acid upon cotarnine, a derivative of narcotine[1]. Some of this substance was placed at my disposal by the late Dr Matthiessen, and an abstract of some of the earlier experiments was published last autumn[2]. Since that time further observations have been made; but they are still imperfect, as the quantity of the poison in my possession was not enough to complete the series of experiments. An attempt to prepare more of the hydrochlorate unfortunately failed. The substance which Dr Matthiessen gave to me was of a dark green colour and very easily decomposed, even by hot water. It was not freshly prepared, and had probably been kept since 1863.

The experiments divide themselves into five groups: (1) the first rough observations; (2) the observations on absorption from the stomach; (3) effects on the motor nerves and voluntary muscles; (4) on the alterations in the blood pressure; and (5) on the regulating nervous system of the heart. The observations here recorded were made solely upon dogs, and upon frogs (*R. temporaria*).

I. The earlier experiments.

A dog, too large for any weighing apparatus to which I then had access, but which much resembled a greyhound in size and stature, had ·2 gr. of the poison injected under his skin on July 22, 1870. The dog took food well, and seemed in his usual health until July 26, when further observations were made. From 11.20 A.M. to 12.15 P.M., ·4 gr. were injected under the skin every 10 minutes, making in all 2·4 gr. injected. No vomiting or defœcation occurred. During the rest of the day, up to 5 P.M., the dog was quite brisk. On July 27, early in the morning, the dog refused the food given him, but in the afternoon he ate biscuit with eagerness and seemed quite lively. But the next day (July 28) the dog was much indisposed to move; there were considerable muscular weakness and difficulty in standing. Flickering contractions passed every few seconds over the more prominent muscles. On July 29, the weakness and flickering contractions were more pronounced. The dog was restless and frequently

[1] Matthiessen and Carey Foster, *Phil. Trans.* 1863, p. 360.
[2] Wickham Legg, *S. Bartholomew's Hospital Reports*, 1870, p. 98.

fell in walking. The water that he took was speedily vomited. The intellect seemed perfect: when caressed, he feebly wagged his tail. At noon the pulse was 152, and respiration 12 in a minute; the pupils were natural. At 3 P.M. the dog was quite unable to rise on his legs. To the hand he felt cold. Pulse 140, resp. 14 in a minute. At 9 A.M. on July 30, the dog was found lying on his side, extended, with eyes half closed and tongue hanging from mouth. At 10.30 A.M. he was insensible to any ordinary stimulus. Pulse 24, resp. 5 in the minute. On pressing the chest, spasms of muscles of trunk came on, lasting about 10 seconds; the muscles of the limbs remained free. The dog died between 12.45 and 1.10 P.M. The examination was made at 1.30. The rigor mortis had not begun, but the animal was quite cold. The muscles were still irritable to the stimulus of the knife. The great veins of the head, chest, and mesentery were full of blood. The right auricle was full of blood: the left comparatively empty. The lungs were slightly hyperæmic. The mucous membrane of the duodenum and of the jejunum for two-thirds of its length was of a deep red colour, much swollen, and too easily torn with the nail. The rest of the organs were natural.

Into a dog weighing 8 lbs., 2·1 gr. were injected at noon on July 7, 1870. Violent vomiting and purging immediately followed. But these soon passed off; and the dog seemed quite well till the evening of July 8, when symptoms similar to those described above came on, and the dog was found dead at 9 A.M. on July 9. In another observation, 2·75 gr. were injected into a dog weighing 15 lbs., at 3 P.M. on July 7. The dog vomited and was purged immediately after: the rest of the afternoon and following day he was quite well; but on July 9, weakness and trembling came on, as described above, and the animal was found dead on the morning of July 11.

In another experiment, a well nourished bitch, weighing about 25 lbs., was used. Observations were made on the temperature, pulse, and respiration. On August 5, 1870, at 11.22 A.M. the temperature in the vagina was 101° F. By some unfortunate forgetfulness the pulse and respiration were not taken. At 11.25, 5 grs. of the hydrochlorate of cotarnamic acid dissolved in 5 fluid-drachms of water were injected under the skin. This was finished at 11.40.

	Temp. in rectum.	Resp.	Pulse.
11.40 A.M.	102°·4	28	96
12.50 P.M.	101°·2	32	160
1.45 "	101°·4	24	152
3 "	101°·6	—	—
3.45 "	101°·8	20	160
4.45 "	102°·2	24	160
5.45 "	101°·6	24	152
6.45 "	101°·6	—	—
7.45 "	102°	—	—

At 11.55 A.M. there were several attempts to vomit: after that time, the dog remained perfectly quiet. At 4 P.M. flickering contrac-

tions of the muscles were noticed; they lasted that evening as long as observation was continued. The power of walking or standing was in no way impaired. Observation was discontinued at 8 P.M.

August 6.	Temp. in rectum.	Resp.	Pulse.
8.45 A.M.	102°·8	24	160
10.10 "	103°·2	—	—
12 noon	103°·4	20	160
2 P.M.	103°·2	20	144
4 "	102°·4	20	148
7.30 "	101°	20	—

In the morning flickering contractions were again noticed. At 2 P.M. the dog when placed on its side was scarcely able to get upon its legs again. At 4.30 P.M., 5 grains of the poison dissolved in 2 fluid-drachms of water were again injected under the skin. A similar dose was repeated at 5 P.M. Since the morning no flickering contractions were noticed.

At 8 P.M. the dog was lying on its side; the limbs retaining whatever position they might be placed in; there was no spasm or rigidity: the intelligence was perfect: the eyes were widely open and followed the movements of the observer. When the cornea was threatened by the finger, the eyelids instantly shut; but the dog might be kicked, the skin of the trunk pinched, or hairs pulled out, without any sign of feeling being made. At 8.30 P.M., T. 99°·6, R. 20, P. 128. At 9.15 P.M. no alteration of this state had occurred; at 10 P.M., T. 99°, R. 14, P. 120. At 10.30 also no alteration. But at 10.45 the limbs became *rigid* when handled; and there was no spasm when the cornea was touched: T. 97°, R. 18, stertorous, P. 120. At 11 P.M. when the chest was gently tapped, *spasms* occurred in body and limbs. At 11.30, T. 94°·8, R. irregular, 4, 5, 6, in successive quarters of a minute, P. 120. At 12 midnight, T. 94°·6. At 12.30 the dog began to howl after vomiting; and at 12.50 A.M. the dog was dead. Immediately after death temperature in rectum was 94°·6 F.

To ascertain the length of time which would elapse between the introduction of the poison and its effects in cold-blooded animals, I injected $\frac{1}{2}$ gr. in solution under the skin of three strong active frogs. No change in the frogs was noticed until the 7th and 8th days, 24 and 48 hours before death. There was at first muscular relaxation, the reflex irritability remaining. On the last day of life the reflex irritability disappeared in all three frogs, while the muscular relaxation was complete. It was easy to produce acute poisoning by injecting $\frac{1}{4}$ gr. into the abdominal cavity. In a few minutes the frogs were under the influence of the poison, and death occurred in from two to three hours. A comparative experiment of injecting an equal quantity of water into the abdomen was unattended by any results.

II. With a view to ascertain if the poison would act if introduced into the stomach, the following experiments were made.

A dog weighing between 20 and 25 lbs. was used. On Oct. 25, 1870, ·5 gr. dissolved in a fluid-ounce of water were injected into the dog's stomach after a fast of 24 hours. No effect was noticed; and on Nov. 1, 2 grains dissolved in the same quantity of water were injected into the stomach. The dog remained in perfect health; so on Nov. 8 at noon, 6 grains dissolved in 2 fluid-ounces of water were injected into stomach. Contrary to my wishes, the animal had been fed at 9 o'clock, and I did not know of this till after the injection had been made. Nothing remarkable occurred till Nov. 19; the dog took food eagerly and seemed quite well. But on the morning of Nov. 19, when the attendant went to take him out of his kennel, he noticed that the dog came out with reluctance, and that his hind-legs were weak. The dog took food, but not with his usual voracity. The pupils acted well to light: but the skin was moved with flickering contractions, much as if the animal were shivering. The next day the dog had lost all these symptoms; and after five days he was sold.

On Nov. 2, 1870, ·3 gr. in solution and suspension were injected into the stomach of a middle-sized active frog. The frog remained active and brisk until Nov. 7, when he was found lying in water with his muscles relaxed. He was put under a bell jar, and next morning (Nov. 8) at 11 was found dead and stiff.

It should be mentioned that all the frogs which were used in the foregoing experiments were taken from a batch of 25 bought in the middle of October. Saving those which, when they were bought, had already received great injury to their limbs, none died from natural causes. About half a dozen were kept till the beginning of February, and they were active and in apparent good health when they were used for some other experiments. From this reason I am inclined to believe that, although the administration of the poison and any apparent results were separated from each other by several days, yet that the symptoms in all the frogs were really due to the absorption of the hydrochlorate of cotarnamic acid. With regard to the dog, it would seem more probable, as digestion was in full action, that the poison was decomposed, and that the symptoms, 10 days after the injection of the poison, although like those following the subcutaneous injection of cotarnamic acid, were caused by some passing indisposition. Further observations on this point would have been very desirable.

III. In order to ascertain the action of the poison upon the motor nerves and voluntary muscles, I made the following

observations upon frogs, employing the method used by Claude Bernard[1] in his experiments on woorara.

Dec. 2, 1870. The ischiatic nerve above the knee of a frog having been isolated and the other soft parts tightly ligatured, ·5 C.C. of a 2 per cent. solution of the poison were injected into the abdominal cavity. In a few minutes the muscles of the frog were perfectly relaxed. Direct irritation of the muscles with Pulvermacher's electrical forceps produced contractions on both sides; those on the ligatured side being a little weaker than those on the free. The same kind of irritation to the nerves produced the same contractions. Electrical irritation of the brain and spinal chord produced active contractions.

The same observations were repeated in another frog with the same results.

These two experiments seem to show that the poison has little influence on the irritability of the ends of the motor nerves; or, when acting acutely, of the voluntary muscles. In Exp. I. the muscles in the dogs were usually found shortly after death irritable to mechanical stimuli: not so, however, the muscles of the frogs; in them the muscular irritability disappeared very shortly after death: for although the animals were examined immediately that the heart's action could be ascertained to have ceased, yet the muscles even then could not be made to contract.

IV. There is, unfortunately, only one observation upon the alteration in the blood-pressure. For the opportunity of making this experiment I am indebted to Dr Sanderson, who, with his usual kindness, placed his kymograph and instruments at my disposal. To my friend and colleague, Dr Brunton, my best thanks are due for the greatest assistance throughout the whole of the experiment; to him indeed is owing all the success which the observation may have attained.

Nov. 25, 1870. Canula in carotid of a bitch weighing 5·3 kilo. Tracheotomy was done at 4.30 P.M.

		Blood Pres.	Pulse in 15″.	Resp. in 15″.
Before the poison was injected		146	—	29
" " " "		126	—	19
4.47′	·02 grm. dissolved in 1 C.C. of water injected into external jugular vein.			
4.47′.6″		76		

[1] Bernard, *Leçons sur les Substances Toxiques*, Paris, 1857, p. 320.

		Blood Pres.	Pulse in 15″.	Resp. in 15″.
4.47′.7″·5	. . . . . .	92		
4.47′.9″·2	. . . .	80		
4.47′.10″·3	. . . . . .	100		
4.47′.11″·3	. . . .	90		
4.47′.22″·3	. . . . . .	·114		
4.47′.25″·8	. . . .	93		
4.47′.33″·2	. . . . . .	118		
4.47′.41″·9	. . . .	80		
4.47′.48″·3	. . . . . .	100		
4.47′.49″·3	. . . .	102		
4.48′.20″	. . . . .	120	—	35
4.48′.50″	. . . .	124	—	24
4.57′	·04 grm. in 2 C.C. of water injected.			
4.57′.15″	. . . . . .	96	—	30
4.57′.41″·6	. . . .	96		
4.57′.44″·2	. . . . .	63		
4.57′.55″·5	. . . .	74		
4.48′.1″	. . . .	80		
4.48′.4″·8	. . . .	62		
4.48′.8″·1	. . . .	56		
4.48′.16″·3	Resp. for last 30″ very irregular . .	67		
4.48′.20″·3	. . . .	58		
4.48′.24″·3	. . . . . .	64		
4.48′.41″·6	. . . .	50	54	14
4·48′.56″·6	. . . . .	44	38	irregular
4.49′.2″·6	. . . .	50		
4.49′.4″·8	. . . . .	32	68	very irregular
4.49′.6″·4	. . . .	24		
4.49′.12″·8	. . . . .	22		
4.49′.14″·1	. . . .	38		
4.49′·17″.4	. . . . .	22		
4.50′.10″·7	. . . .	32		
4.51′.10″·7	Pressure sank towards the end of 60″ .	32—6		
4.51′.40″·7	Pressure highest in middle of 30″ . .	20—26		
4.52′.40″·7	Pressure sinking slowly	10—18		
4.53′.40″·7	. . . . .	10—12	—	7
4.54′.40″·7	. . . .	14—20	—	
4.55′.40″·7	. . . . .	14	—	7
4.56′.40″·7	. . . .	13—18	—	7
4.57′.10″·7	Pressure steadily rising	18—30	—	7
4.57′·40″·7	. . . . .	30—46	—	10
5.29′	. . . .	104	—	12
5.30′	·04 grm. in ·2 C.C. of water injected.			

	Blood Pres.	Pulse in 15″.	Resp. in 15″.
5.34′	93	—	12
5.37	100	—	14
5.47′	94	—	18
5.49′	80	—	18·5
5.49′. 30″	102		
5.50′	73		
5.50′. 40″	105		
5.50′. 50″	86		
5.51′	80		
5.51′. 20″	102		
5.51′. 38″	100	—	17

In this experiment it was only occasionally that the number of heart-beats could be counted. There was a notable diminution of the blood-pressure as soon as the poison was injected into circulation: and this became extreme between 5 min. and 6 min. after the second injection, the pressure sinking to 10 and 12 mm. After the third injection the blood-pressure varied from 105 to 73 mm., rising and falling in continuous curves, but showing no such decided drop as after the first or second injections.

V. This experiment upon the vagus is simply a continuation of the preceding Exp. IV.

		Blood Pres.	Pulse in 15″.	Resp. in 15″.
5.57′	Section of vagus . .	106	—	24
5.57′. 15″	. . . .	90		
5.57′. 45″	. . . .	106		
6.3′	Right vagus irritated by induced current.	—		
6.4′. 30″	Irritation stopped . . 7 Respirations in 1′. 30″.	—	40	
6.21′	. . . .		45	20

Irritation was now again applied to vagus. Respiration ceased; but the heart continued to beat 45 times in 15″.

The vagi of the frogs used in Exp. III. were prepared and irritated by means of the electrical forceps. No alteration in the frequency of the heart-beats happened.

These experiments show that, after poisoning by this substance, irritation of the cardiac end of the divided vagus produces no change in the number of the heart's contractions.

Results:—(*a*) After the injection of 2 to 2·75 grs. of the hydrochlorate of cotarnamic acid under the skin of dogs, death

follows in from 36 to 72 hours, symptoms first appearing 24 to 36 hours after the injection.

(*b*) Acute poisoning has no effect upon the irritability of the ends of the motor nerves, or of the muscles themselves.

(*c*) The blood-pressure is greatly diminished.

(*d*) The cardiac branches of the vagus are paralysed.

The long interval between the injection of the poison and the appearance of symptoms is not peculiar to the hydrochlorate of cotarnamic acid. Dr Letheby[1] has noticed the same phænomenon after the administration of nitrobenzole to dogs and cats. "The time that elapsed from the administration of the poison to the coming on of the first symptoms... varied from nineteen hours to seventy-two—in most cases it was about two days[2]." The long interval is probably due to slowness of absorption, since in Exp. IV. when the substance was injected directly into the blood, obvious effects of the poison were observed in a few seconds.

It would have been more satisfactory if all these observations could have been confirmed by repeated experiment; and much more light could have been thrown upon the action of this curious poison, had a greater supply of the substance been at hand. Unfortunately, also, the paper in the *Philosophical Transactions*, already quoted, does not seem to contain directions for the preparation of the substance which are sufficiently full. Several attempts have been made during the past winter to prepare the hydrochlorate of cotarnamic acid; but they have all failed: and until the poison shall have again been prepared, the points suggested by the foregoing observations must remain in obscurity.

[1] Letheby, *Proc. Roy. Soc.* Vol. XII. p. 550. "On the Physiological Properties of Nitrobenzole and Aniline."

[2] Letheby, *op. cit.* p. 554.

ON THE CONSTRUCTION AND USE OF A SIMPLE CARDIO-SPHYGMOGRAPH. By A. H. GARROD, *St John's College, Cambridge.*

IT is evident that a precise knowledge of the intervals between the main elements of the cardiograph and the sphygmograph trace must be of value in studying the hydrodynamics of the circulation of the blood; and a description will be here given of an instrument by which several results of interest have been obtained on this subject.

This cardio-sphygmograph consists of a piece of board, ten inches long by five and a half inches broad, and is about half an inch thick, along one side of which one of Marey's sphygmographs can be fixed, as shewn in the accompanying figure. On

Fig. I.

the opposite side to this is a spring (*a*), similar to that employed in the sphygmograph, which is attached to a moveable support (*b*), so that its strength may be modified. A small ivory pad (*c*) is fixed to the lower surface of the free end of the spring, and this is in communication with the recording lever of the cardiograph apparatus by means of a silk thread (*d*). In this instrument the cardiograph lever (*e*) is very light, a little over two inches long, and connected to the board by means of a frame (*f*), which is just free of the moveable part of the sphygmograph, when that is in position. The lever, which is one of the third system, is connected on either side, close to its fixed end, to two silk threads, one of which (*d*) is attached to the cardiograph spring, and the other to a small

spring (*g*), which moves it when it is less acted on by the stronger one. The apparatus is so arranged that the lever works perfectly when it is so placed as to be above the recording paper of the sphygmograph, when the latter is in position. The tip of the lever carries a steel pen (*k*).

The apparatus therefore consists of a cardiograph and of a sphygmograph, and these are so fixed that they both record on the same paper; and the object to be attained is to get them both to record at the same time, the one the movements of the heart's apex, the other the dilatations of the artery at the wrist.

To obtain this result the sphygmograph is first fixed, as usual, on the *left* arm, and the recording paper is adjusted to its place on the watchwork. With the cardiograph in the right hand, the left arm is then moved until the attached instrument rests on the board in the position shewn in the figure, and when there, it is maintained in its place by certain pegs and holes in the board, which respectively come in contact with the main parts and receive the projections of the instrument.

The arm and attached instruments are then moved until the pad of the cardiograph spring is brought in contact with the spot, generally between the 5th and 6th ribs, at which the heart's pulsations are most marked; the position of the pad in relation to the board having been previously so fixed as to enable this to be done with facility, the whole being maintained in the horizontal position.

The contact of the cardiograph pad with the chest wall causes the lever to recede from the chest, and it is allowed to do so until its pen arrives above the recording paper; the whole apparatus being steadied by the right hand. When the levers of the two instruments are both found to be moving freely, the watchwork of the sphygmograph is set in action by a string (*m*) held by the right hand, and at the other end connected with the stop-block of the train of wheels. The two levers recording on the smoked paper give a combined trace of which Figure II. is an example.

As with simple cardiograph traces it is advisable and almost necessary to hold the breath while the trace is being taken, and further, to simplify the working of the instrument, the chest should be empty at the time.

Fig. II.

It is evident from the above description that the two levers write in opposite directions, and consequently this figure must be turned the other way up that the cardiograph trace may be properly seen, and then it must be read from right to left, not from left to right, as the sphygmograph trace.

The commencement of the two traces is indicated by the curved lines to the left of each trace as they are looked at without moving the page, and these curved lines are produced by letting the levers move without the watchwork, whilst the instrument is being fixed in position. Synchronous points in the two traces must evidently be at equal distances from the starting points in the traces, and therefore the one can be projected on the other by compasses or by superposition.

In all cases it is necessary, both in the cardiograph and in the sphygmograph trace, to project all the main points, such as the origin of the main rise, and the deepest point in the secondary fall, on to one line in the trace; for, as the levers move in part of a circle, any point at the summit of the trace, if projected straight downwards, would not be correctly related to the lower parts of the trace. This correction is best made by a most simple arrangement; a flat piece of board has a straight slip of wood fixed close to one edge; against this the tracing rests, being supported on the board. Two nails are fixed on the board, so that they bear the same relations to its supported trace as that did to the axes of the levers which marked on it in the cardio-sphygmograph. The marking apparatus consists of two pieces of string, each fixed at one end to the nails, and at the other carrying needles; these pieces of string must be of the same length as the levers to which they correspond, the points of the needles must pierce them, and the other ends of the needles must be attached to the nails by a thread to prevent them from moving irregularly.

The cardio-sphygmograph can be best applied when the

person using it is sitting, as it can then be made to rest on the arm of a chair, and in practice it is better not to have the main part of the instrument press against the chest wall, as if it does the heart's movement imparts itself to the whole apparatus, and so complicates the trace.

In considering the results arrived at by the use of this instrument, it will be necessary to define a few of the terms that have to be employed in explaining them.

(1) The first cardiac interval is that which occurs between the commencing systolé and the closure of the aortic valve at the heart.

(2) The first arterial interval is that which occurs between the indications of the commencing systolé and the closure of the aortic valve in an artery. The radial artery at the wrist is the only one that is here considered.

As the commencement of the arterial rise is somewhat later than the commencing systolé at the heart, and as the difference between the first cardiac interval and the first arterial interval is not great, these two events coincide in part of their duration, and give rise to minor divisions, which may be thus named and defined.

(3) The first cardio-arterial interval is that which occurs between the commencing systolé at the heart and its indication in an artery (the radial).

(4) The conjugate cardio-arterial interval is that which occurs between the commencing systolic rise in an artery and the closure of the aortic valve at the heart.

(5) The second cardio-arterial interval is that which occurs between the closure of the aortic valve at the heart and its indication in an artery[1].

On comparing the lengths of the first cardio-arterial interval with different rates of pulse, it is found that as the pulse is slower, so this interval is longer, and that its length does not increase as rapidly as the pulse beat, but as its square root.

[1] In the above definitions it has been assumed that the sphygmograph trace gives indications of the closure of the aortic valves; and in the measurements to be referred to below, the secondary rise, which puts so abrupt a termination to the major fall in each pulsation, is considered to be caused by the closure of these valves, as generally assumed; though Dr Sanderson has arrived at a different conclusion (*Medical Times and Gazette*, March 25, 1871), from evidence which seems to be anything but convincing.

Consequently if the number of times that the first cardio-arterial interval is contained in its component pulsation is represented by z and the rapidity of the pulse by x, then $xz = k \,.\, \sqrt{x}$, and measurements shew that the constant quantity k equals 39 (or perhaps 39·25) for the sitting posture. A knowledge of this equation, therefore, gives a means of calculating the length of the first cardio-arterial interval when the rapidity of the pulse is known; and as the first cardiac interval also varies as the square root of the pulse beat[1], it is evident that from its definition the first cardio-arterial interval must be a constant part of the first cardiac interval, whatever the rate, and this has been found to be the case by independent measurement. Another necessary result from these equations is, that the conjugate cardio-arterial interval varies inversely as the square root of the heart's rapidity.

The length of the second cardio-arterial interval can also be found by subtracting the length of the conjugate cardio-arterial interval from that of the first arterial interval, which varies inversely as the cube root of the pulse rate[2], and by this means it has been found to vary very little with different rapidities of pulse, being a little longer in the slower pulses.

In the sphygmograph traces of slow pulses the major descent of the first arterial interval is broken by a notch, and it is found that the deepest point of this notch is always exactly synchronous with the point of closure of the aortic valve. This leads to the almost necessary conclusion, that the subsequent slight rise or change in direction of the trace is the result of the simultaneous movement of the whole column of blood produced by the shock of the closure of the aortic valve; the secondary rise at the commencement of the second arterial interval being the more slowly transmitted pressure wave, which started at the same time. This explanation being correct, it is evident that the results obtained, by measuring the number of times that the interval between the origin of the main arterial rise and the bottom of the notch in the major fall is contained in the first arterial interval, ought to give the same results as

[1] *Journal of Anatomy and Physiology*, Nov. 1870. "On Cardiograph Tracings from the human chest wall."

[2] *Proceedings of the Royal Society*, No. 120, 1870. "On the relative duration of the component parts of the radial sphygmograph trace in health."

those obtained by dividing the calculated length of the conjugate cardio-arterial interval into the first arterial interval. Such has been found to be the case very closely, but a sphygmograph trace must be a very good one to shew the notch in the first arterial interval sharply defined, and the subsequent rise commencing abruptly.

In all the cases above discussed it has been assumed that the subject from whom the traces were taken was sitting at the time, and as the length of the first cardiac interval changes for the same rate of pulse with change of position, it is evident that the equation given above (namely, $xz = 39\sqrt{x}$) must be changed also; and the change probably consists in increasing the value of the constant for the standing posture, as then the first cardiac interval is shorter, but still varies inversely as the square root of the heart's rate.

The explanation given above of the cause of the notch in the first arterial interval, might lead to the expectation that the commencing cardiac systolé indicates itself at the wrist in the same way; but there is no such marked change of direction in sphygmograph traces, though a slight rise is generally seen just before the main ascent originates, especially in pulse of about 70 in a minute; and it is not improbable that the second rise in the extremely dicrotic pulse of adynamic pyrexia is caused by a combination of the slow pressure wave resulting from the closure of the aortic valve, and the sudden onward motion given to the whole mass of blood in the vessels at the moment of opening of the aortic valve at the commencement of the next systolé.

The facts on which the above equations have been based are published in the *Proceedings of the Royal Society*, Vol. XIX., No. 126, p. 318.

Mr Hawksley, of Blenheim Street, is constructing a cardio-sphygmograph from the model described above, with a few minor improvements, which can easily be applied in the study of pathological conditions.

ON THE TELSON OF THE MACRUROUS CRUSTACEA. By A. H. GARROD, *St John's College, Cambridge.* (Pl. XI. Fig. 5.)

THE relations of the telson of the lobster and its allies are so variously regarded by zoologists of the present day, that no apology is needed in bringing forward any facts which tend to settle the point. By Milne-Edwards it is considered as a seventh abdominal segment; but I cannot find in his writings any reasons given for his belief. Van Beneden is also stated by Prof. Rolleston to hold the same opinion. Prof. Huxley considers the telson an azygos appendage, and not a true segment of the body; and Prof. Rolleston agrees with him, stating that it only carries appendages in one or two cases, whereas it is a law common to all Crustacea, that every segment has its appendages.

An attempt will be here made to shew that in a specimen of Scyllarus arctus, in the Zoological Museum of the University of Cambridge, there is sufficient evidence to prove that the telson is a true body segment, and that it is provided with true segmental appendages, though the nature of these is somewhat modified by cohesion and adhesion.

In this specimen the *sixth* abdominal segment is in the main similar to that in the lobster, but its dorsal surface is grooved instead of plain. The infero-lateral terminations of its dorsal shield are slightly recurved and not sharply pointed, those of the first abdominal segment being decidedly so, but the acumination becomes less marked in each succeeding one. The swimmerets are greatly developed, the propodite not exhibiting any decided spinous protuberances. Both the exo- and the endopodite, which are expanded horizontally, are composed of two, a proximal calcified and a distal fin-like portion; the anterior margin of the former in each of these segments being prolonged outwards in the form of a spine. The distal fins are composed of a translucent membranous substance supported on a radiating framework. At the attached extremities of the anterior and posterior margins of these fins there are small elongated calcified masses, which seem to be the points at which

their delicate structures come in contact with one another and the neighbouring parts.

Ventrally there is a transverse calcified bar, concave forward, slender and composed of a central and two lateral elements, which are fixed to the sides of the dorsal shield.

The *seventh* segment, or telson, consists of a distal fin and a proximal calcified portion. The fin is azygos, semi-elliptical, and supported on rays, like the exo- and endopodites of the sixth segment. It also resembles them in having two lateral elongated calcified masses at its margins. There is no median separation.

The calcified portion consists dorsally of a small semicircular centrum or dorsal shield, which is close under the dorsal plate of the preceding segment, and is separated from the lateral masses by a marked groove, not by an articulation.

These lateral masses are each distinctly separable into three parts: 1st, a thin longitudinal one, which approaches the sixth dorsal plate, and ends posteriorly in a sharp spine. Internally it comes in contact with the next, the 2nd part, which latter joins the azygos centrum at its antero-internal margin, and at its distal end gives attachment to the 3rd part. Beyond the centrum there is a considerable interval between the two lateral masses dorsally.

The 3rd part consists only of a free spine, directed backwards, and articulated to the 2nd. It rests on the azygos fin.

Ventrally this segment presents only a short transverse bar, in front of the anus, composed, as in the preceding one, of a central and two lateral portions.

On the supposition that this seventh segment is a true one, the small dorsal centrum corresponds with the much larger one in the other segments, while the swimmerets are represented by the lateral masses described above; the 2nd part of which corresponds with the propodite, and the 1st and 3rd with the exo- and endopodite respectively, each bearing spines, and connected with the propodite and with the fin.

The short transverse ventral bar corresponds to the narrow centrum, and is composed of three portions, as in the preceding segment. I have no means of telling whether it is laterally connected with the dorsal shield.

The position of the anus behind this seventh transverse bar is strongly in favour of the alimentary canal being segmentally terminal, although this condition is disguised by the coalescence of the lateral appendages above, which consequently makes the anus ventral.

If the above explanation is accepted with regard to this particular species, it is highly probable that it is true in those allied to it, and would then refer to the lobster and cray-fish. Prof. Rolleston says of the latter[1], that the proximal segment of the telson is not calcified continuously across its ventral surface, as the other segments are; but this appearance would arise from the small size of its real centrum, which, as in Scyllarus, would be quite anterior, and the coalescence in the middle line of the proximal portions of the endopodites of the segment. In the telson of the lobster, by looking at its dorsal surface obliquely, a system of undulations is observed, which corresponds, though in a much more disguised form, with the condition in Scyllarus, the two extreme lateral pieces with the accompanying spines being easily seen, and the small elevated centrum at the anterior part.

In conclusion, an attempt has been made to shew, (1) That in some Macrurous Crustacea, and therefore probably in all, the telson is a true body segment with lateral appendages, which are modified by cohesion and adhesion, (2) That the body of the segment terminates posteriorly in front of the anus, and that the cohesion of the endopodites superiorly causes the anus to be ventral.

[1] *Forms of Animal Life*, 1870, p. 95.

OBSERVATIONS IN HUMAN ANATOMY. By W. W. WAGSTAFFE, B.A., F.R.C.S., *Demonstrator of Anatomy at St Thomas's Hospital.*

ALMOST as a matter of course it must happen that the rough anatomy taught in the dissecting room admits of very little correction, for careful teachers have worked at the same subject ages before us and with the same means as we now employ. In minuter details, which are now submitted to investigation with the aid of instruments and reagents unknown to our predecessors, there is a much wider field for the discovery of new facts, and it is in this field that nearly all workers are now employed. But it has been remarked in this Journal (Vol. III. p. 1) by Professor Wood, that "occasionally some out of the way corner of a science remains after years of investigation the retreat of a curious error which has escaped the detection of successive generations of observers," and it is to some of these out of the way corners that the accompanying remarks apply. I have no doubt that other teachers have noticed some of the facts which I now bring forward, but I am not aware that they have been made public or at all generally admitted. I have no doubt too that many inaccuracies have escaped my observation, and I trust we shall have the experience of other demonstrators upon points in which they have observed the text-book descriptions to be incomplete or faulty.

Pectoralis major. The arrangement of the fibres of the Pectoralis major has usually been dwelt upon in the dissecting room and in manuals of Anatomy as a peculiar feature in the muscle, and one of which there are not many examples in the body —that of the complete twisting, so to speak, of its fibres. I do not find, however, that the descriptions at present given of the manner in which the fibres are arranged are borne out by the examination of a large number of subjects.

In the seventh edition of Quain's *Anatomy*, the work which must undoubtedly be looked upon as the standard English work on the subject, I find it stated—"The arrangement is such that at last the folded tendon of the muscle is inserted into the humerus in two nearly parallel lines which are connected below.

The fibres of the clavicular part in the order of their origin from without inwards are prolonged into the anterior or outer line of insertion in order from above downwards. The pectoral part of the muscle is inserted into the inner or posterior line; the fibres which have the highest origin being the lowest at their insertion, and those which arise lower in succession from the chest passing higher and higher to their insertion on the humerus."

In the third edition of Gray's *Anatomy* by T. Holmes, it is stated that "the upper fibres overlap the middle, and the middle the lower portion, . . . the tendon of insertion consists of two laminæ, placed one in front of the other and usually blended together below. The anterior, the thicker, receives the clavicular and upper half of the sternal portion of the muscle; the posterior lamina receiving the attachment of the lower half of the sternal portion. From this arrangement it results that the fibres of the upper and middle portions of the muscle are inserted into the lower part of the bicipital ridge; those of the lower portion into the upper part."

In Ellis's *Demonstrations of Anatomy* the description is similar to that in Gray, but decidedly more clearly expressed.

The description of the actual arrangement would more correctly be given in the following manner.—

The fibres of the Pectoralis major are arranged in two sets —a clavicular and a sternal set, and between the two there is, in many cases, a well defined cellular interval: in all cases the separation is appreciable. They run to their insertion upon the outer edge of the bicipital groove of the humerus in the following manner. The fibres of the clavicular portion are inserted in the same order as they arise—the outermost, and particularly the deeper fibres of origin, are inserted at the uppermost part of the tendon, the innermost fibres, or those nearest to the sterno-clavicular articulation, pass down to the lowermost part of the tendon. The sternal portion is peculiarly arranged. It will be seen on turning aside the clavicular part that the uppermost fibres of the sternal muscle, *i.e.* those from the first costal cartilage (if, as is perhaps most usual, there is attachment to that part), or from the neighbourhood of the sterno-clavicular articulation, pass downwards under cover of the cla-

vicular muscle, and are inserted into the uppermost part of the second plane of attachment to the outer bicipital ridge. Fibres from the sternum opposite the first intercostal space, or sometimes a little lower, can be traced down to be inserted into the lowest part of the attachment to the humerus. Below these last-mentioned fibres the muscular bundles gradually pass under the previous ones, and ultimately the lowest fibres of origin from the aponeurosis of the external oblique pass to the highest part of the third plane of insertion, and it is these fibres which are connected with the fibrous band passing upwards to the head of the humerus.

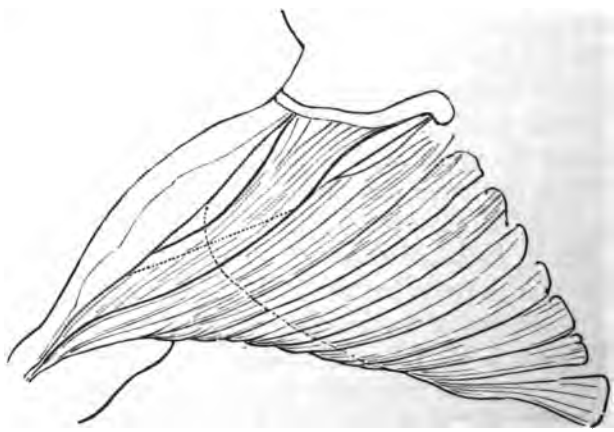


Diagram shewing arrangement of fibres of Pectoralis major in man. Clavicular set passing down to anterior plane of insertion, upper sternal set to second plane, lower sternal set (from about first intercostal space downwards) passing to third or deepest plane of insertion; the lowest fibres of the set inserted highest up on humerus.

It will thus be seen that the so-called tendon of the Pectoralis major is composed of three layers, 1, that of the clavicular position, 2 and 3, those of the sternal portion doubled upon itself below but easily separable above. Of these layers the first and second have usually about the same extent of attachment above and below, but the third passes considerably higher than the others.

Flexor tendons of the fingers and toes. The attachment of these tendons to the phalanges has been accurately described

by Weitbrecht in his *Syndesmologia*, and more recently by Professor Marshall in the *Medico-Chirurgical Review* for 1853; and the latter anatomist has pointed out the existence of the vincula subflava, and ascribed to them the function of passively drawing back the flexor tendons after the latter have been pulled towards the hand in flexion of the fingers. But a very important function of the ligamenta brevia appears to have had but little stress laid upon it. These ligaments pass backwards from the tendons close to their insertion (and this is most easily demonstrated in the case of the ligamentum breve of the flexor sublimis), and are attached to the depression below the head of the previous phalanx—that of the flexor sublimis into the first, that of the profundus into the second phalanx. They are therefore not brought into play until the phalanges are very considerably flexed. It will be seen then, in the case of the flexor sublimis for instance, that the proper insertion of the tendon and the ligamentum breve are equally acted upon by further action of the muscle, and the first phalanx in this case is now flexed. Extreme action might otherwise be attended by a dislocation of one phalanx upon the other, and it appears certain that the result of the peculiar arrangement of the ligamentum breve will be to obviate this tendency to dislocation.

Internal pterygoid. The internal pterygoid muscle arises from the pterygoid fossa, from the inner surface of the external pterygoid plate and from that part of the palate-bone which fits in between the two plates. A portion of the origin of this muscle has however usually escaped description in our textbooks of Anatomy. In every case in which I have noticed the attachment of the muscle carefully another portion of it arises from the outer surface of the tuberosity of the palate-bone just where that bone is continuous with the external pterygoid plate. This portion of the muscle will therefore be related rather peculiarly to the external pterygoid muscle in overlapping it, whereas the main part of the internal pterygoid is hidden in its origin by the fibres of the external pterygoid.

Peroneus tertius. This muscle is described in Quain, Gray, and Ellis as arising from the *lower fourth* of the fibula on its anterior surface, besides being connected with the interosseous membrane and with a fibrous aponeurosis between it and

the peroneus brevis. In my experience the peroneus tertius has been, where separable in its origin from the extensor longus digitorum, almost always connected much more extensively with the fibula. The extent of surface of the fibula occupied by the muscle has usually exceeded the lower half of the bone, not including however the part below the tibio-fibular articulation. Not infrequently the origin of the muscle has occupied the lower two-thirds of its anterior surface, and then the proportionate length of attachment of the extensor longus and of the peroneus tertius has been a matter of surprise to the student. I have also frequently been able to trace the lower muscular fibre of the extensor as lying on a plane internal to those of the peroneus, and attached under cover of the latter to the anterior surface of the fibula down to just above the tibio-fibular articulation.

Sacrolumbalis. This portion of the erector spinæ is usually described as passing upwards to be attached to the lower ribs—Quain says six or seven, Ellis the same, and Gray the lower six. My respected colleague, Mr Rainey, has, however, for years taught in the Dissecting Room at St Thomas's that slips can without difficulty be traced from the muscle into every one of the ribs; and this I have verified in almost every subject in which I have examined the attachments of this muscle carefully. It is well known that the muscles of the back vary very much in the number of slips of which they are composed, but it can be demonstrated readily that the Sacrolumbalis has a much larger extent of attachment to ribs than is described in our text-books of Anatomy.

Transversalis colli et capitis. Under this head Mr Ellis proposed to include the transversalis colli and trachelomastoid, and it is a matter of surprise that in the last edition of Quain's *Anatomy* advantage is not taken of this means of simplifying the very complicated description of the muscles of the back. One of our objects as teachers of Anatomy should undoubtedly be to render the explanation of these muscles more intelligible to students, and more easily retained in their memory. As it is, students rather shirk the dissection of the muscles of the back on account of the difficulty of remembering the attachments, the formidable array of names, and the number of

so-called separate muscles; and they are hindered also very materially by reason of the difficulty of distinguishing them as separate structures. The transversalis colli and transversalis capitis (trachelomastoid) are commonly one muscle at their origin, but divide towards their insertion into a neck portion (tr. colli) and a head portion (tr. capitis), resembling in this respect the splenius: and the analogy between these two muscles can be traced even further. The *splenius* arises from spinous processes in the dorsal region down to about the sixth, from the vertebra prominens, and from the ligamentum nuchæ in the neck about as high as opposite the third cervical vertebra. The transversalis arises from transverse processes in the dorsal region down to about the sixth, and from articular processes in the neck about as high as the third.

It will be useful to notice here the attachment of the complexus as compared with that of the above two muscles. The *complexus*, including the biventer cervicis, which is practically a part of the complexus, arises from transverse processes in the dorsal region as low as about the sixth (though it not infrequently receives fibres from much lower down) and from articular processes in the neck about as high as the third cervical vertebra. It will thus be seen that a comparison of these three muscles, grouped in the manner I have indicated, simplifies the description usually given. The extent of origin of each is about the same, but the splenius arises from spinous processes or their equivalent, the ligamentum nuchæ, while the transversalis and complexus rise from transverse processes or their equivalents, articular processes. The splenius and transversalis are single muscles at their origin but divide into head and neck portions, which receive names indicating their destination (spl. capitis, spl. colli, transv. capitis, transv. colli).

Supinator radii longus. The action of this muscle is so little that of supination that it does not deserve the name given to it. In Quain's *Anatomy* it is admitted that its chief use is to flex the forearm, but it is stated that supination having commenced by other muscles the movement is assisted by the supinator longus. In Gray's *Anatomy* it is stated that the supinator longus and brevis are the supinators of the forearm and hand, the longus more especially acting where the limb

is pronated: when supination has been produced the further action of this muscle is to flex the forearm.

It may be seen, upon examining the action of the muscle in the dead body, that it cannot be considered more of a supinator than of a pronator. When the hand is made prone the supinator longus brings up the radius over the ulna to the semi-prone position. When the hand is supinated it brings up the radius again over the ulna also to the semi-prone position. After this the muscle flexes the forearm. In other words, its tendency is to throw the forearm and hand into the position it readily occupies when placed across the chest as in a sling. Dr Duchenne has recently employed the interrupted current for the purpose of demonstrating upon the living subject the action of different muscles, and shewed that by placing the two poles of the battery over the body of the supinator longus when the hand was lying supine, complete pronation was produced; but there is no evidence that the stimulus did not affect other muscles besides the supinator. On the contrary, I am of opinion (having been the subject experimented on by Dr Duchenne at a recent demonstration at this hospital) that the stimulus was so powerful as to affect at least those lying directly under it, namely the radial extensors of the wrist. Now it will be seen, upon examining the attachments of these muscles, that their action will undoubtedly be that of pronation provided the hand be originally supine: for they pass downwards from the outer condyle to the back of the wrist, and will naturally tend when contracting to bring the back of the hand over and pronate the hand distinctly.

Popliteal space. In giving the boundaries of this space I am not aware of any notice being taken of the adductor magnus. This muscle is placed deeply on the inner side of the space above the condyle, and upon raising the semi-membranosus can be readily discovered. It is that portion of the great adductor especially which is passing down as a strong tendinous structure to the inner femoral condyle. The boundaries, as I have been in the habit of teaching them, are as follows: on the inner side above, the Adductor magnus, deeply placed and hidden by the Semi-membranosus, upon which is lying the long tendon of the Semi-tendinosus with some of the fleshy part of the muscle.

Superficial to these muscles, and not directly in relation with the popliteal space, are the Gracilis and Sartorius. On the outer side above is the Biceps, both its heads being visible. Below we have on the inner side Gastrocnemius, and on the outer side Gastrocnemius and Plantaris.

Acromio-thoracic artery. The description of the origin of this vessel in all our manuals of Anatomy is, I am inclined to believe, inaccurate. It is stated to arise from the axillary artery in its first part, from that portion which lies between the first rib and the upper border of the pectoralis minor, but in every subject in which I have examined the artery during the seven years I have been demonstrator in the anatomical department of this Hospital I have found it rising from the second part of the axillary artery. I am indebted to Mr Rainey for first pointing out this fact to me. It is given off from the front of the axillary artery a short distance under the pectoralis minor and then passes upwards to appear in the space above the muscle, where it gives off its pectoral and humeral branches as usually described.

TWO CASES SHOWING A PECULIAR ARRANGEMENT IN THE FIBRES OF THE EXTERNAL PTERYGOID MUSCLE IN MAN. By W. W. WAGSTAFFE, B.A., F.R.C.S., *Demonstrator of Anatomy at St Thomas's Hospital.*

Two similar cases of rather curious muscular abnormality have recently come under my notice in the Dissecting Room at St Thomas's Hospital, and as they are of a character not hitherto described, it may be interesting to have them recorded.

Case 1.—Upon the outer surface of the right external pterygoid muscle, close to its origin from the margin of the spheno-maxillary fossa, I noticed, in demonstrating upon this subject, a number of vertical aponeurotic fibres. These aponeurotic fibres were connected above with the pterygoid ridge of the

sphenoid along its anterior part, and ended below in well-defined muscular bundles. The muscular bundles were found to be attached below to the lower part of the outer surface of the external pterygoid plate. These superficial vertical fibres of the external pterygoid consequently ran a short distance from bone to bone without a possibility of movement of either extremity. It therefore became a matter for conjecture as to what function they could possess, and the further connections of this structure were carefully examined. At the anterior border of the muscle it was seen that the usual origin from the tuberosity of the upper maxilla and palate-bone was entirely deficient, and with this, and probably accounting in a measure for it, there was a rather larger spheno-maxillary fossa than is commonly observed. The most posterior of the superficial aponeurotic fibres gradually blended with the rest of the muscle in the manner shown in the accompanying sketch, and the uppermost of these passed directly into the anterior surface of the inter-articular fibro-cartilage of the joint. Upon turning back

Case 1.

Showing the vertical tendinous fibres on the external pterygoid, from the under surface of which a portion of the horizontal muscular fibres sprang. The band of vertical fibres is about half-an-inch broad. For the lower fourth of their length they usually became muscular.

the vertical aponeurotic fibres it could then be seen that from their under surface started a large number of the ordinary horizontal fibres of the muscle.

Here then seemed to be the explanation of what appeared an anomaly. These vertical fibres, half tendinous, half muscular, running between two fixed points of bone formed a start-point for other muscular fibres at right angles to them. The use of the muscular portion of these vertical fibres seemed to be to render tense the tendinous portion at exactly the same time as strain was exerted upon it by the contraction of the horizontal fibres rising from it.

The subject from whom this was taken was certainly not well developed muscularly in other parts, and here the fibres were soft and fatty.

Case 2.—This was very similar to the first case. A band of aponeurotic fibres (about ½ inch broad) ran vertically downwards upon the outer surface of the right external pterygoid close behind the superior maxilla. These fibres were attached

Case 2.

Vertical band of fibres on surface of external pterygoid, about half-an-inch broad, the upper three-quarters usually tendinous, lower quarter muscular and attached to lower part of external pterygoid plate, the most posterior fibres running into the internal pterygoid. Horizontal fibres of external pterygoid rise from the under surface of the anterior fibres of the band.

above to the pterygoid ridge in its whole length, below they became muscular, but the development of muscle in connection with their lower ends was not so well seen as in the previous case. The anterior fibres ended below by being inserted into the outer surface of the external pterygoid plate. The posterior ended by joining some of the fibres of the internal pterygoid. From the under surface of the anterior fibres sprang the ordinary horizontal fibres of the external pterygoid, as in Case 1, but the posterior fibres passed freely over the external and joined the internal pterygoid. The attachment of the external pterygoid to the back of the superior maxilla and palate-bone was in this case also deficient, but the spheno-maxillary fossa not so large as in the previous case. No similar structure existed on the other side, but the origin of the muscle was normal.

In each of these cases a peculiar musculo-tendinous structure was present upon the surface of the external pterygoid; in each this structure was mainly composed of tendinous fibres, but muscular tissue was found in its lower portion; in each the structure was placed at right angles to the direction of the fibres of the muscle upon which it was lying; in each the ordinary horizontal fibres of the muscle took origin from its under surface; and in each these horizontal fibres had not their usual attachment to the back of the superior maxillary and palate-bones. The result obtained by this peculiar structure in each case was the same; the tendinous portion gave origin to the superficial fibres of the external pterygoid, while the muscular portion rendered the former tense at the time they were being pulled upon by the fibres rising at right angles from them.

It would be interesting to observe how far such an arrangement of superficial aponeurosis exists in the external pterygoid of animals, and particularly in those animals in whom the spheno-maxillary fossa is proportionally larger than in man. I have not had, however, the opportunity of making any observations on other animals, and can therefore only hope to see the experience of other observers recorded in some future numbers of the *Journal*, and to give my own as occasion offers.

RESEARCHES ON THE ACTION OF CERTAIN DRUGS UPON THE URINE, AND ON THE INFLUENCE OF DIET AND MENTAL WORK UPON THIS EXCRETION. (A Thesis for Graduation in Medicine, at Edinburgh, 1870.) By JOHN WILSON PATON, M.D., Edinburgh; M.R.C.S., *Rockferry, Cheshire.*

INTRODUCTION.

THE object of my researches has been to determine

I. The influence of various diets upon an individual whose urea excretion is shown to have been steadily above the average.

These experiments and analyses were conducted in the laboratories of Professors Christison and MacLagan with the utmost minuteness; and their results have enabled me to discuss a few points in connection with the condition of so-called "Azoturia."

II. The influence of the preparations of Broom Tops, and their active principles, Scoparine and Sparteia, upon the excretion of urine.

III. The influence of mental rest and severe mental work, upon the excretion of urea and several of the other urinary ingredients. The experiments in this part were conducted on two individuals, and

IV. A short epitome of the researches which have tended to alter our opinions regarding the production of urea, will be given, and the arguments which have led me to believe in the doctrine, which ascribes the formation of this substance to the processes going on in the liver.

I am much indebted to Professors Christison and MacLagan, for their kindness in allowing me the use of their laboratories for these investigations.

I have also gratefully to acknowledge the invaluable assistance I obtained from Dr Arthur Gamgee, who not only taught me the methods of quantitative analyses of the urine, and corroborated any of my analyses which appeared doubtful, but also

gave me the benefit of his advice, in every difficulty, and especially when considering the question of diet.

I have also to thank Dr Thomas R. Fraser for the kind assistance he gave me during the preparation of Scopărine.

PART I.

CHAPTER I.—*On the influence of various diets upon the excretion of urine.*

These analyses were commenced on February 1st, 1867, when I was in perfect health, my primary object being, to ascertain the effects of Broom Tops, upon the urine; but, finding my excretion of urea higher than normal, the investigations with Broom Tops were delayed till March 14th, the intervening time being occupied with the question of diet.

Before the first of February my diet was general and unrestricted; enough being taken to maintain the weight of the body. There was a preference for nitrogenous food—meat, eggs, cheese, bread, milk, and butter, being the principal articles of food, and the quantity was regulated by the feeling of satisfaction. Potatoes were taken in very small quantity, and sugar was entirely left out.

On February 1st, I put myself on a diet, containing 237·65 grains of Nitrogen and 3219·624 grains of Carbon[1], and commenced the analyses of urine. Every article of food was carefully weighed, and its nutritive value afterwards calculated from the Tables of Dr Parkes[2]. This diet was insufficient, but no great desire for any more was experienced. The urine[3] was normal in quantity and density, but the amount of urea was above that given as the average for healthy men[4], being 715·765 grains daily, and containing 334 grains of Nitrogen[5].

Supposing that diet No. I. was too large, its nutritive value not having been calculated at that time, a reduction was made from the 6th till the 12th of February inclusive[6].

[1] See Table I. p. 301.
[2] *Manual of Practical Hygiene*, by Ed. A. Parkes, M.D., 1864, p. 141.
[3] See Table V. p. 304.
[4] Average for healthy men 520 grains, Dr Ed. Smith. *Proceedings of the Royal Society of London*, May 30, 1861.
[5] See Table VIII. p. 307.
[6] See Table II. p. 302.

This diet contained only 205·245 grains of Nitrogen, and 2879·78 grains of Carbon.

This reduction had a marked effect on the excretion of urea, so that it fell in one day from 752 grains to 560 grains[1]. But this diminution was temporary; and from the 8th till the 10th of February it rose steadily, after which there was a slight fall. The daily average of urea, during this period, was 655·3 grains, containing 305·8 grains of Nitrogen[2].

This reduction in diet and the maintenance of a high excretion of urea—the daily excreta containing 100 grains more Nitrogen than the ingesta—led to loss of weight and strength, syncope almost supervening on two occasions.

The appetite throughout this time was ravenous, and at bedtime the feeling of hunger was intense, but did not prevent my sleeping well, often awaking in the morning in the position in which I had lain down to rest.

On February 13th a more abundant diet, almost identical with that of Feb. 1st, was begun, containing 239·5 grains of Nitrogen and 3455 grains of Carbon[3].

The excretion of urea was slightly lowered by this[4]. I was however losing flesh and strength daily, being now six pounds lighter than when the experiments were begun.

On February 15th some additions were made to the last diet, raising its value to 338·87 grains of Nitrogen and 5219·6 grains of Carbon[5].

Under this diet all the symptoms of weakness rapidly passed off. At first it was felt rather too much, perhaps from the excess of Carbon; but after a few days, it could be taken with relish, and was continued with more or less relish, during the rest of the experiments.

From the 15th till the 28th of February inclusive, the urine averaged 1556 cc. daily. Its colour was normal, and reaction always acid. The amounts of Uric acid, of Chloride of Sodium, and of anhydrous Sulphuric and Phosphoric acids were normal[6].

The urea was 711·392 grains daily, containing 332·3 grains

[1] See Table V. p. 304.
[2] See Table VIII. p. 307.
[3] See Table III. p. 302.
[4] See Table VIII. p. 307.
[5] See Table IV. p. 303.
[6] See Table VI. p. 305.

of Nitrogen, and could frequently be precipitated with nitric acid without evaporation[1].

During this period there was a slight excess of Nitrogen in the ingesta, compared with the egesta, and a corresponding slight increase in weight, viz. 1¾ lb. in fourteen days.

This diet may be said to suit me exactly, there being a difference of only 6·572 grains of Nitrogen between the daily ingesta and egesta.

The fæces were not examined, but the value of the observations is not diminished thereby; for Dr Parkes has shown from examination of the fæces, that nearly all the nitrogen ingested leaves the body as urea[2]. In one of his cases the difference was similar to mine, being only 6·503 grains daily.

During this period my health was excellent, and I felt able for any work, corporeal or mental. I walked twice daily from the west end of Princes' Street, to and from the Infirmary, analysed my urine, and frequently sat up till midnight, or even 2 A.M., reading. There was no headache, thirst, weakness or sleeplessness, but there was occasionally slight constipation.

For this I took, on and after March 1st, 300 cc. of water *primo mane*. This had not much effect on the bowels, but increased the quantity of urine and urea[3], and an abnormal condition was to a slight extent occasioned; for while the water probably accelerated tissue change, I did not wish to disturb the experiment by adding to a weighed diet. Weight was therefore lost, more Nitrogen passing as urea than the ingesta contained, and I was not in such continual good health as on the same diet without water. The only other effects noticed, were a slight increase in the amounts of Sulphates and Chlorides, and a great decrease in the amount of Uric Acid[4].

Dr Parkes has given the limits of the nutritive value of diets, for men of mean height, weight, and activity, as follows[5]; from 250—350 grains of Nitrogen daily, and from 3500—5000 grains of Carbon daily.

[1] See Table VIII. p. 307.
[2] *Proc. of the Royal Soc. of London*, Vol. XVI. p. 44. *On the elimination of Nitrogen, during rest and exercise, on a regulated diet*, by E. A. Parkes, M.D.
[3] See Tables VII. and VIII. pp. 306, 307.
[4] See Table VII. p. 306.
[5] *Manual of Practical Hygiene*, p. 137.

The first three diets[1] upon which I experimented are therefore under the *lowest* of his calculations, and insufficient to maintain health; intense hunger and prostration of strength, amounting almost to syncope, resulting from their use.

Under the first and second diets about 100 grains of Nitrogen passed daily as urea, more than the ingesta contained[2]. Where did it come from? Undoubtedly from the absorption of some albuminous substance. I was being slowly starved, and my tissues were supplying the amount of Nitrogen required for physical and vital work.

Under the third diet, although the Nitrogen of the ingesta was increased 34 grains daily, the Nitrogen of the egesta still fell, but more slowly, 8 grains daily[3]. Why? Because, while the amount of Nitrogen ingested was still insufficient by 100 grains daily, the Nitrogen added prevented such a rapid waste as would have gone on for a little longer, had diet No. II. been continued.

Immediately the fourth diet was begun the symptoms of starvation vanished, my usual health and strength were regained, and weight increased.

For the first two days of this sufficient diet (Feb. 15th and 16th) 29·6 grains of Nitrogen[4] were daily retained in the system to make up for the loss already sustained; and during the continuance of this diet till the 28th of February, the amount of Nitrogen excreted fluctuated slightly[5] on the whole, a little less being passed than was taken into the system[6], and weight consequently gained. From the 20th till the 28th the weight remained stationary. The increase of weight was therefore during the first five days (15th—20th), and more especially on the first two days, when a large quantity of Nitrogen was retained.

From March 1st to 13th, under the same conditions and upon the same diet, with the addition of 300 cc. of water, *primo mane*, 1½ lb. weight was lost, the Nitrogen of the egesta exceeding that of the ingesta[7].

What was the cause of this loss of weight? Had the water

[1] See Table VIII. p. 307.
[2] See Table VIII. p. 307.
[3] See Table VIII. p. 307.
[4] See Table VI. p. 305.
[5] See Table VI. p. 305.
[6] See Table VIII. p. 307.
[7] See Table VIII. p. 307.

anything to do with it? I believe it had; for while under the same diet for fourteen days, but without this excess of water, weight was not only *not* lost, but even gained at first, and then remained perfectly stationary, immediately the water was commenced the urea excretion rose[1] and weight was lost.

Now the averages of the urine from the 15th till the 28th of February may be taken as my normal excretion, and those from the 1st till the 15th of March may be looked upon as the result of an excess of water[2]. This excess of water was only relative[3], and if the diet had not been limited by weight there should probably have been an increase of ingesta to counterbalance the action of the water.

Now if my normal daily excretion of Nitrogen be 332 grains[4], how was it, that on a diet containing 100 grains less Nitrogen than I required[5], the average daily excretion was 334 grains? and why was the first rapid fall in the excretion of Nitrogen[6] followed by a slower decrease[7]?

Here I hold with Voit[8], that the primary large excretion, is due to the destruction of an easily oxidised albuminoid substance, most probably existing in the blood, and which is much more amenable to oxidation than the "stock-albumen" existing in the tissues, and on which an animal undergoing starvation comes at length to live; and the succeeding slow fall in the excretion of Nitrogen, is accounted for by supposing, that this albuminoid substance is nearly used up, and that the tissues are supplying the necessary Nitrogen from the stock-albumen which they contain.

There were other minor changes in the urine during the time water was taken[9]; viz.

1. A slight increase in the amount of Chlorides, estimated as Chloride of Sodium.

[1] See Table VII. p. 306. [2] See Table VII.

[3] It is only 58 cc. beyond the quantity allowed by Moleschott for healthy adults. Moleschott, *Phys. der Nahrungsmittel*, 1860, p. 223. Parkes, *Man. Pract. Hygiene*, p. 139.

[4] See Table VIII. p. 307.

[5] See Tables I. and VIII. pp. 301, 307.

[6] See under Diet II. Tables V. and VIII.

[7] See under Diet III. Tables V. and VIII.

[8] Voit, *Zeitschrift f. Biol.* II. pp. 307—365. *Centralblatt*, 1867, No. 7. *Journal of Anat. and Physiology*, Vol. II. p. 182.

[9] See Table VII. p. 306.

2. A considerable increase in the amount of Sulphates, estimated as Anhydrous Sulphuric Acid.

3. A *marked* decrease in the amount of Uric Acid.

CHAPTER II.—*On so-called Azoturia.*

Formerly urea was regarded chiefly in relation to pound weight of body, and the amount of urea normally excreted per pound weight was considered to be about 3·53 grains in adults and 5·77 grains in children. At present urea is not so much regarded in relation to pound weight. It may, however, be very important when the relation of urea to the amount of albumen consumed in the liver, and the amount of carbonaceous food required by muscles in their action is known.

It is, however, important to notice, that while the normal excretion is 3·53 grains, my excretion was 4·55 grains per pound weight when I was in perfect health, from Feb. 15 to Feb. 28, inclusive.

Is this to be regarded as a diseased in addition to being an abnormal condition, and classed under those cases of Prout's and Willis', and called by the latter "Azoturia"[1]?

I consider this supposition to be a very questionable one; several well marked cases of excess of urea in the urine are recorded, in which crystals of the nitrate of urea could be obtained at once, as in my case, without evaporation, and a certain series of symptoms has in nearly all these cases been noticed[2]. With a healthy florid complexion, their complaints have been constant and urgent—flatulence, acidity, extreme languor, restlessness at night, and sometimes great nervousness being complained of. Even moderate exertion caused extreme fatigue, and in some there seemed to be a marked indisposition to, and incapacity for mental exertion. The urine was frequently abundant, with constant desire to void it by night and by day, dull pain in the back, and in some occasional irritation at the neck of the bladder, no thirst, no inordinate

[1] *The nature and treatment of Stomach and Renal Diseases*, by Wm. Prout, M.D., 1848, pp. 93—103. *Urinary Diseases and their treatment*, by Robt. Willis, M.D., 1838, p. 18. *On the composition of the Urine in Health and Disease and under the action of Remedies*, by E. A. Parkes, M.D., 1860, pp. 377 and 374.

[2] Symptoms abridged from several cases. See Paper by W. H. Fuller, M.D. *Lancet*, 1867, Vol. II. p. 705; or in *Transactions* of the Royal Med. and Chirurgical Soc. of London, Nov. 26, 1867.

appetite, no affection of the skin or pulse existed, but in nearly all there was more or less gastro-intestinal derangement.

Here, undoubtedly, we have some abnormal conditions superadded to a normal large excretion of urea. In some, nervous symptoms from the existing dyspepsia were predominant, in others a urinary irritation, and in a few there seems to have been a slight diabetes insipidus. But in no case recorded does it appear to me justifiable to conclude that the excess of urea is a disease. In some it is only the normal condition of the individual found out when he is under treatment for something else, while in others it may be only a symptom of disturbed hepatic function.

In support of the first of these assertions I may say, that no two analysts agree concerning the absolute amount of urea, or the relative amount to body weight, which healthy men may excrete, the range per pound weight being from 2·31 grains to 5·82 grains:

TABLE A.

Table showing the amount of Urea excreted per pound weight, according to different observers. From Carpenter's *Human Physiology*, 1869.

Age.	Urea per lb. weight in grains.	Observer.
22	2·94	Scherer.
28	3·00	
31	3·69	Rummel.
65	2·01	
45	2·44	Bischoff.
42	2·76	Smith.
...	3·72	
...	5·82	
20	4·52	Urine of A. C., Table IX.
26	3·343	Urine of A. G., Table XI.
21	4·546	Urine of J. W. P.

That my analyses of the urine of A. C.[1], a florid healthy individual, aged 20 years, weighing only 120 lbs., show a similar relation to body weight as my own analyses, being 4·52 grains per pound weight; that not one of the symptoms recorded is applicable to him or to myself, with the exception of having a florid complexion; that since these analyses were made, three years ago, my health has been excellent, though in the meantime I have had scarlet-fever, followed by slight inflammation (erysipelatous) of one foot, one slight attack of muscular rheumatism, and a severe attack of facial neuralgia; and instead of having any languor or muscular debility, I have for more than a year, whilst visiting patients, walked from six to ten or twelve miles daily[2].

Again, in healthy children there is an excretion of three times as much urea per pound weight as in adults[3]; while as we advance in years, and the various tissues begin to lose their normal activity, and more especially in old age, when they may be diseased or almost exhausted, we find that the excretion of urea greatly diminishes.

Is not, therefore, a large excretion rather a sign of activity of function and health of tissue, and diminution of the excretion an evidence either of old age or disease of that organ in which urea is formed?

These arguments are supported by Voit's experiments on animals during starvation[4]; for he found that the more perfect the condition and nourishment of the animal at the commencement of the experiment were, the higher was the primary excretion of urea, and his experiments are further corroborated by my urea excretion under diets I. and II[5].

[1] See Table IX. p. 307.

[2] Last accounts from A. C., who is now in the West Indies, state that he is in good health.

[3] *Chemistry in relation to Physiology and Medicine*, by G. Day, M.D., 1860, pp. 41, 42.

[4] *Zeitschrift Biol.* II. pp. 307—365; *Journal of Anat. and Phys.*, Vol. II. p. 182.

[5] See Tables V. and VIII. pp. 304, 307.

PART II.

ON THE INFLUENCE OF THE PREPARATIONS OF BROOM TOPS UPON THE URINE.

CHAPTER I.—*On the Influence of Scopărine.*

This drug not being used in medicine, could not be obtained pure at any chemist's. I therefore made some by the long process described by Stenhouse[1], obtaining it in a perfectly pure amorphous state, and of a pale lemon yellow colour. The experiments were continued for six days (March 14 to 19 inclusive), on a weighed diet[2], and the urine of twenty-four hours examined daily. One hundred and five grains were taken in all.

On the first day ten grains were taken in two doses. From the second till the fifth day inclusive, fifteen grains were taken in three doses; and on the sixth day thirty-five grains were taken in three doses. Each dose was dissolved in a little warm water, a few drops of the aromatic spirit of ammonia being added to aid its solution. The following are the results obtained[3]:

1. The quantity of urine was *not* increased.
2. The density of the urine was not altered.
3. There was no perçeptible influence on the amounts of chloride of sodium, anhydrous sulphuric acid, phosphoric acid or uric acid.

Therefore, although highly recommended in dropsies[4], Scoparine has *no* diuretic effect in health. No other effect was observed.

CHAPTER II.—*On the Influence of an alcoholic extract of Broom Tops.*

An ounce and a half of Broom Tops was boiled in alcohol, and an extract obtained by evaporation. This was divided

[1] John Stenhouse, M.D., *Philosophical Transactions*, 1851, Vol. CXLI. p. 422.
[2] See Diet No. IV. with water, Table IV.
[3] See Tables XIV. and XV. pp. 312, 313.
[4] Recommended by Mead, Cullen, Pearson, Pereira, Stenhouse and Arthur Mitchell.

into thirty-six pills. Six pills correspond to the largest dose of infusum scoparii[1]. The experiments lasted three days, from March 20 to 22 inclusive, under the same conditions as when Scoparine was taken.

Six pills were taken on the first day in three doses. Twelve pills were taken on the second day in three doses. Eighteen pills were taken on the third day in three doses. The results were similar to those of Scoparine—negative[2]. No other effect was observed.

CHAPTER III.—*On the influence of Sparteia.*

These experiments lasted four days, from March 24 to 27 inclusive, under the same conditions as before. The Sparteia was kindly given me by Professor Christison.

Not knowing the effect of this drug, very small doses were taken at the commencement. Four grains of Sparteia were made up into thirty-two pills with thirty-two grains of ext. of gentian, and thirty-two grains of pulv. glycyrrhizae. Each pill contained one-eighth of a grain of Sparteia. The doses were taken at 8 A.M., 12·30 P.M., and 4·30 P.M.

March 24. One pill taken at each of the above hours. Slight headache all day.

March 25. Two pills were taken three times. There was slight headache all day. Shortly after the second dose the headache was very severe. There was no giddiness, but for a quarter of an hour there was a sensation of tingling in the left hand and foot. I felt weak and disinclined to walk on account of my legs being unable to support me as usual. These effects were not noticed after the third dose. At 10·30 P.M. I felt well and as strong as ever.

March 26. Nine pills were taken to-day in three doses. During the whole day there was a slight headache, and I felt and appeared to be ill. The tingling sensations in the extremities continued.

March 27. Twelve pills taken in three doses. Debility and slight tingling sensations continued.

[1] *Brit. Pharmacop.* 1867, p. 100.
[2] See Table XIV. p. 312, and Table XV. p. 313.

Stenhouse, from the observations of Dr Arthur Mitchell of Glasgow[1], considers Sparteia a powerful narcotic poison, inferior to nicotia and conia, and producing in animals excitement, intoxication, coma, slight convulsions, death. The headache and tingling sensations are undoubtedly due to its narcotic properties. It had no effect on the quantity of the urine or any of its ingredients[2].

CHAPTER IV.—*On the influence of Infusum Scoparii.*

These experiments lasted six days, from March 28 till April 2, 1867. The infusion was that of the *British Pharmacopœia*, 1867 (page 100).

On March 28, eleven and a quarter ounces were taken.

March 29, twelve ounces were taken.

March 30, sixteen ounces were taken.

March 31, seventeen ounces were taken.

April 1, sixteen ounces were taken in two doses.

April 2, sixteen ounces were taken in two doses.

On deducting the additional water, taken as infusion, from the urine passed, no influence is noticed in the quantity or ingredients of the urine[3]. During all these experiments with Broom Tops, I lost weight slightly, more nitrogen passing in the urine than the ingesta contained. The daily loss was equal to nearly three-quarters of an ounce of albuminoid substance.

From these experiments I conclude that the preparations of Broom Tops, *per se*, their alkaloid Sparteia, and their so-called active principle Scoparine[4], have no specific effect in increasing the quantity of urine of healthy men.

PART III.

ON THE INFLUENCE OF SEVERE MENTAL WORK UPON THE EXCRETION OF URINE.

These experiments were made in September and October, 1867, and lasted nine and twelve days. The urines analysed

[1] *Philosophical Transactions*, Vol. CXLI. p. 422.
[2] See Table XIV. p. 312, and Table XV. p. 313.
[3] See Table XIV. p. 312, and Table XV. p. 313.
[4] Stenhouse, *op. citat.*

were those of Dr Arthur Gamgee and myself. We were in excellent health both at the beginning and end of the analyses. The analyses were divided into periods.

In the 1st, or Rest period, as little mental work as possible was done.

In the second, or Work period, the mental work increased daily.

In the third, or Second Rest period, as little mental work as possible was again done.

We were both on weighed diets[1]. The urine was always collected for twenty-four hours[2]. The amounts of exercise and sleep were as nearly as possible the same during the three periods. On comparing the tables[3], the results are nearly similar in both of us, and the following conclusions may be drawn:

1st. That prolonged mental work increases the quantity of urine. In Dr Gamgee it rose from 1352 to 1515 cub. cent. In J. W. P. it rose from 1724 to 2021 cub. cent.

2nd. That the density does not correspondingly fall. In Dr G. it rose from 1018 to 1023. In J. W. P. it remained stationary at 1023.

3rd. That the amount of Nitrogen passing off by the kidneys is increased by mental work. In Dr G. it rose from 206·5 to 223·1 grains. In J. W. P. it rose from 337·5 to 380·1 grains.

4th. That the amount of chlorides is greatly increased In Dr G. it rose from 8·8 to 10·5 grms. In J. W. P. it rose from 13·9 to 16·7 grms.

5th. That the amount of phosphoric acid is not only not increased, but even slightly diminished. In Dr G. it fell from 3·29 to 2·75 grms. In J. W. P. it fell from 3·6 to 3·58 grms.

On again returning to a period of comparative rest (period No. III.) it is observed,

1st. That the amount of nitrogen is greatly diminished: In Dr G. it fell from 223 to 182 grs. In J. W. P. it fell from 380 to 240 grs.

2nd. That the chlorides are not sensibly altered.

[1] Diet of A. G., Table X. p. 308. Diet of J. W. P., Table IV. p. 303.
[2] Urine of A. G., Table XI. p. 309. Urine of J. W. P., Table XII. p. 310.
[3] See Table of Urinary Averages, Table XIII. p. 311.

3rd. That the phosphoric acid is slightly increased in Dr G., and unaltered in quantity in J. W. P.

4th. That both of us lost weight slightly.

How can we explain these facts, which in every important point coincide in both of us. It is well known, that under various mental conditions the amount of urine is increased, and I have already pointed out, that if in health the urine be increased by drinking water, or by some static influence, the urea is also increased[1]. Now, under mental work it is the same—given an increase of urine, an increase of urea is the result. The urea has no relation to the mental work except in so far as the latter influences the excretion of water. Under this perverted nervous action a general wash out takes place and the urea and chlorides are therefore increased.

In proof of this, if we look back to table VI. we find that on Feb. 24 there was a large excretion of water with increase of urea and chlorides; and again, while reading hard, from March 11th till March 13th, there was an increase in the quantity of urine, urea, and chlorides[2].

These experiments prove that the theory of the increase of phosphates in the urine under mental work is erroneous. In fact, it looks as if prolonged mental work rather decreased them, as if the brain during action did not only *not* lose phosphoric acid, but even required it for its action. If so, phosphoric acid has a similar relation to mental work, as nitrogen has to muscular work.

The loss of weight in both of us was due to more nitrogen passing off than the ingesta contained. This was especially so on the 29th, when the urine of both of us rose from some cause, and contained a very large amount of urea and chlorides; Dr Gamgee passing 104 grains of nitrogen more than his diet contained[3], and J. W. P. an excess of 43·648 grains[4]. This rise in the amount of urine and urea is so marked in Dr Gamgee's case that this day has not been included in the calculations, as

[1] See p. 288 for the effect of water. See also Dr Smith's Paper in the *Proc. Roy. Soc., London*, May 30, 1861, "On the influence of Barometric pressure and temperature on the excretion of Urine."

[2] See Table VII. p. 306.

[3] Compare Tables X. XI. XIII.

[4] Compare Tables IV. XII. XIII.

it immediately preceded the work period, and would most probably have falsified the results; and because late in the previous evening and during the early part of the night his brain was in an unusually active condition.

PART IV.

ON THE PHYSIOLOGICAL RELATIONS OF UREA.

Until lately the old theories of the production of urea from excess of nitrogenous food, and oxidation of muscle, muscular force resulting from that oxidation, were believed.

In 1866, Professors Fick and Wislicenus[1], from the results of their experiments on ascending the Faulhorn on a purely non-nitrogenous diet, concluded that the nitrogenous parts of muscles do not waste during action, and that urea is not the measure of intra-muscular changes during action. Since then Dr Parkes has corroborated their view, that the evolution of nitrogen is not a condition of muscular action[2]. Their experiments tend strongly to support the theory that muscular force is derived from the oxidation of hydro-carbonaceous materials in muscle, and Dr Frankland's[3] experiments on the amount of potential energy in various articles of diet corroborate this, for he found that the amount of energy stored up in hydro-carbonaceous substances was much greater than that in the nitrogenous. If, then, urea does not proceed from the oxidation of muscle, whence does it come?

Zalesky[4], having denied the presence of uric acid in the blood of birds, Meissner[5], from analyses, proved its presence in that fluid, and as he found it in much larger proportion in the liver than in any other tissue, obtaining as much as ·31 grm. from 500 grms. of liver, he gave it as his opinion that uric acid was formed in the liver.

[1] *On the origin of Muscular Power*, by Dr A. Fick and Dr J. Wislicenus, Zurich. *London, Edin. and Dub. Phil. Mag.* 1866, p. 485.
[2] *Proc. Roy. Soc. Lond.*, Vol. XVI. p. 44.
[3] *Lond. Edin. and Dub. Philosph. Mag.*, Sept. 1866.
[4] *Centralblatt*, 1865, p. 202. *Journ. Anat. and Phys.*, Vol. III. p. 239.
[5] Henle und Pfeuffer's *Zeitschrift*, 13, XXXI. pp. 144—223. *Journ. Anat. and Phys.*, ut supra.

Formerly it was stated by Heynsius and Stokvis that urea existed in the liver[1], and Meissner has now shown that it exists in the livers of both the carnivorous and herbivorous animals in much larger proportion than was believed, obtaining as much as ·09 grm. from 474 grms. of dog's liver, and ·025 grms. from 347 grms. of rabbit's liver. He therefore advanced the theory that urea is formed in the liver, and he is supported by the following facts:

1. It has never been found with certainty but in the liver, blood and urine.

2. Frerichs[2] and Städeler state, that in the urine of acute yellow atrophy of the liver the urea is not to be found, its place being taken by leucine.

3. Again, Harley has failed to detect it in cases of chronic atrophy of the liver[3].

4. Vogel states that it is diminished in carcinoma of that organ.

5. In some cases, where the liver is much disordered with fœtid high-coloured urine and no febrile state of system, urea is increased.

These facts undoubtedly point to the liver as being the factor of urea, and it seems reasonable to suppose that so large and important an organ should have some other function than merely to excrete a few ounces of bile daily. That other function has been already in part known as its glycogenic function[4]. I say *in part*, for it is only now that another part is known.

It was long known that the liver of animals fed entirely on animal food, secreted sugar. Now, as animal food contains no starchy elements from which that sugar could be formed, it was argued, and then proved by experiment, that the liver had the power of making sugar. Bernard[5], supported by Lehmann[6], believed that the sugar was formed from albuminous compounds, because there was less albumen in hepatic than in

[1] *Journ. Anat. and Phys.*, ut supra, p. 240.
[2] *Clinical Treatise on Diseases of the Liver*, by Dr Frerichs, Vol. I. p. 220. New Sydenham Soc. Edition.
[3] *Prin. of Human Physiology*, Carpenter, p. 463.
[4] Bernard, *Archives Générales de Médicine*, 1848. Carpenter, *op. cit.* p. 436.
[5] *Op. cit.*
[6] Carpenter, *op. cit.* p. 436. *Compt. Rendus de l'Acad. des Sciences*, 1855, p. 587.

portal blood, and they thought that the albumen was split up in the liver into the sulphuretted biliary acids and sugar. Then, however, the old theories of the production of urea were held. Now they are known to have been wrong. If the liver form bile and sugar, two hydro-carbonaceous substances—the former containing only a very small per centage of nitrogen—out of highly nitrogenised albuminous compounds, what becomes of the 15½ per cent. of nitrogen? *It goes to the formation of urea, urea being formed in the liver from the splitting up of albumen.* Whether it be at once formed or only produced after many changes, is at present unknown. The latter is the more probable[1]. Schultzen and Neubri of Berlin[2] believe that it passes at least through leucine and glycocol, and in animals whose urea excretion is constant, they have found that the increase of urea is in exact proportion to the amount of glycocol given with their food.

TABLE I.

DIET No. I. FEBRUARY 1—5, 1867.

	Water.	Water-free food.	Albuminates.	Carbohydrates.	Fats.	Salts.
Meat	187·50	62·50	37·50	0·	21·0	4·0
Bread	124·0	186·0	24·80	152·52	4·68	4·03
Eggs	82·32	29·68	15·12	...	12·99	1·12
Butter	1·80	28·20	0·90	...	27·30	...
Potatoes	111·0	49·0	2·25	35·10	0·15	1·50
Milk	358·44	53·56	16·48	20·60	15·24	2·47
Total in Grammes	665·06	408·94	97·05	208·22	81·36	13·12

The albuminates contain 15·4 Grms. = 237·65 grains of Nitrogen.

The albuminates, carbohydrates and fats contain 208·633 Grms. = 3219·624 grains of Carbon.

[1] See the decrease in the amount of uric acid from taking water, p. 288 and Table VII.

[2] *Academy*, Jan. 8, 1870.

TABLE II.

DIET No. II. FEBRUARY 6—12.

	Water.	Water-free food.	Albuminates.	Carbohydrates.	Fats.	Salts.
Milk	268·83	40·17	12·36	15·45	11·43	1·854
Butter	1·80	28·20	0·90	...	27·30	variable.
Egg	41·16	14·84	7·56	...	6·496	·56
Meat	187·50	62·50	37·50	...	21·0	4·0
Potatoes	118·40	41·60	2·40	37·44	·16	1·6
Bread	116·0	174·0	23·20	142·68	4·35	3·67
Total in Grammes.	733·69	361·31	83·92	195·57	70·736	11·684

The albuminates contain 13·3 grms. = 205·245 grains of Nitrogen.

The albuminates, fats and carbohydrates contain 186·61 grms. = 2879·78 grains of Carbon.

TABLE III.

DIET No. III. FEBRUARY 13 and 14.

	Water.	Water-free food.	Albuminates.	Carbohydrates.	Fats.	Salts.
grms.						
Egg 56	41·16	14·84	7·56	...	6·496	0·56
Butter 30	1·80	28·20	0·90	...	27·30	...
Meat 250	187·50	62·50	37·50	...	21·0	4·0
Potatoes 250	185·0	65·0	3·75	58·50	·25	2·5
Milk 412	358·44	53·56	16·48	20·60	15·24	2·47
Bread 395	158·0	237·0	31·60	194·34	5·92	5·13
Tea & Water	1230·	...	...	...	...	...
Total in grms.	2161·9	461·1	97·79	273·44	76·206	14·66

The albuminates contain 15·52 grms. = 239·5 grains of Nitrogen.

The albuminates, fats and carbohydrates contain 223·926 grms. = 3455 grains of Carbon.

TABLE IV.

Diet No. IV. February 15 onwards.

	grms.	Water.	Water-free food.	Albuminates.	Carbohydrates.	Fats.	Salts.
Eggs	168	123·48	44·52	22·68	...	19·488	1·68
Butter	30	1·80	28·20	·90	...	27·30	...
Butcher meat	250	187·5	62·5	37·5	...	21·0	4·0
Potatoes	250	185·0	65·0	3·75	58·5	0·25	2·5
Milk	712	628·44	83·56	25·48	32·3	22·74	3·97
Bread	475	190·	285·	38·00	233·7	7·12	6·17
Cheese	30	11·04	18·96	10·05	...	7·29	1·62
Sugar	45	1·35	43·65	...	43·42	...	·22
Tapioca	40	...	40·0	...	40·0	...	...
Tea and Water		1230·	...	...	...	...	...
Salt	12	...	12·428	...	...	...	12·428
Total in grms.		2558·61	683·819	138·36	407·92	105·188	32·588

The albuminates contain 21·96 grms. = 338·87 grains of Nitrogen.

The albuminates, fats and carbohydrates contain 338·23 grms. = 5218·9 grains of Carbon.

TABLE V.

Urinary Analyses under Diets No. I. II. and III.

Diet.	Date.	Quantity.	Density.	Urea, per Cent.	Urea. Grms.	Urea. Grains.	Calculated Solids. Grms.	Calculated Solids. Grains.	Grammes. Na Cl	Grammes. Po_5	Grammes. So_3	Grammes. Uric Acid.	Weight.
	Feb.	C. C.		Grms.									
	1	1450	1029	3·117	46·073	710·917	100·8	1555					
	2	1830	1024	2·613	47·831	738·041	104·7	1615					Jan. 21.
I.	3	1890	1023	2·511	47·462	733·348	103·5	1597					$160\frac{3}{4}$ lbs.
	4	1100	1030	3·792	41·717	643·691	79·1	1220					=72·78 kils.
	5	1700	1024	2·870	48·790	752·829	97·3	1501					
	6	1730	1024	2·101	36·351	560·905	99·0	1527					
	7	1275	1031	2·767	35·285	544·457	94·9	1464					
	8	1040	1035	4·253	44·239	682·607	87·7	1353					
II.	9	1040	1034	4·510	46·904	723·728	85·1	1313	...	...	...	...	$157\frac{3}{4}$ lbs.
	10	1150	1030	4·253	48·918	754·806	82·7	1276					=71·41 kils.
	11	1100	1035	3·997	43·974	678·529	92·8	1432	...	2·631	...	·5016	
	12	1450	1023	2·870	41·615	642·119	79·4	1225					
III.	13	1530	1022	2·665	40·774	629·13	80·1	1236	9·79	2·781	...	...	$154\frac{3}{4}$ lbs.
	14	1430	1024	2·921	41·773	644·57	81·8	1262	10·098	2·96	...	...	=70·05 kils.

TABLE VI.

Urinary Analyses under Diet No. IV.

Diet.	Date.	Quantity.	Density.	Urea, per Cent.	Urea.		Solids.		Grammes.				Weight.
					Grms.	Grains.	Grms.	Grains.	Na Cl	Po_5	So_3	Uric Acid.	
	Feb.	C. C.		Grms.									
	15	1100	1026	3·69	40·59	626·303	68·33	1054	8·58	3·108	...	...	...
	16	1700	1023	2·665	45·305	699·056	93·19	1438	13·6	...	...	...	155½ lbs.
	17	1320	1027	3·638	48·031	741·126	82·5	1273	9·24	...	...	...	= 70·81 kils.
	18	1561	1025	3·117	49·6	765·339	93·2	1438	12·48	3·244	...	...	...
	19	1490	1025	3·126	46·581	718·746	88·9	1371	12·665	2·813	2·156	·445	...
	20	1509	1026	2·972	44·855	692·112	93·7	1445	...	...	...	...	156½ lbs.
IV.	21	1500	1025	2·972	44·587	687·985	88·5	1365	12·	2·712	2·658	·502	= 71·27 kils.
	22	1700	1021	2·665	45·305	699·056	84·8	1308	12·75	3·03	2·54	...	...
	23	1700	1024	2·921	49·661	766·273	97.3	1501	13·26	3·575	...	...	...
	24	2190	1020	2·357	51·639	796·639	104·0	1604	17·52	3·377	...	·662	...
	25	1380	1026	2·972	41·02	632·945	85·7	1322	11·59	2·633	...	...	...
	26	1420	1026	3·28	46·904	724·728	88·2	1361	12·58	3·657	3·12	·632	...
	27	1560	1027	2·818	43·972	678·495	97·5	1504	14·66	...	...	...	156½ lbs.
	28	1650	1025	2·870	47·355	730·687	98·5	1520	...	3·717	...	...	= 71·27 kils.

TABLE VII.

Urinary Analyses under Diet IV. with Water.

Date.	Quantity.	Density.	Urea, per Cent.	Urea.		Calculated Solids.		Grammes.				Weight.
				Grms.	Grains.	Grms.	Grains.	Na Cl	Po_5	So_3	Uric Acid.	
March 1	C. C. 2275	1020	2·152	48·969	755·597	108·1	1668	...	3·727	3·215	·750	
2	1900	1020	2·562	48·687	743·248	90·3	1393	12·201	3·638	...	...	
3	2120	1018	2·716	57·584	888·528	90·5	1395	...	4·124	...	...	
4	1775	1022	2·716	48·213	743·933	92·9	1433	...	3·362	3·026	...	156¼ lbs.
5	1850	1022	2·767	51·198	789·996	97·9	1510	12·395	3·504	...	...	= 71·09 kils.
6	2020	1022	2·460	49·692	766·747	105·8	1632	12·834	3·619	...	...	
7	1912	1021	2·665	50·954	786·232	95·5	1473	...	3·719	3·466	...	
8	1650	1025	2·870	47·355	730·687	103·6	1597	...	3·125	...	·1264	
9	2450	1026	2·306	56·503	871·843	152·2	2348	13·321	3·512	...	...	
10	1865	1025	2·921	54·481	840·646	111·3	1717	12·055	4·105	3·596	...	
11	2150	1018	2·460	52·890	816·092	91·7	1415	...	3·302	...	...	155 lbs.
12	2265	1021	2·357	53·390	813·921	113·1	1745	13·764	3·942	...	...	= 70·45 kils.
13	1960	1024	2·870	56·252	876·968	112·2	1731	14·664	3·813	...	·1608	

TABLE VIII.

AVERAGES OF INGESTA AND EGESTA UNDER THE VARIOUS DIETS.

Diet.	Water taken.	N. in Food.	C. in Food.	Urine.	Density.	Urea.	Total N. in Urea.
I.	1965	grains. 237	grains. 3219	C. C. 1594	1026	grains. 715	grains. 334
II.	1763	205	2879	1255	1030	665	305
III.	2161	239	3455	1480	1023	636	297
IV.	2558	338	5219	1556	1024	711	332
IV. with Water.	2858	338	5219	2014	1021	801	373

TABLE IX.

ANALYSES OF URINE OF A. C.

Date.	Quantity.	Urea. Grammes.	Urea. Grains.	Urea P. C.	Weight.
Dec. 2	C. C. 1350	29·7	458·33	grams. 2·2	120 lbs. 10 oz. = 54·7 kils.
3	1850	46·25	713·37	2·5	120 lbs. 11 oz.
4	1400	35·0	540·12	2·5	
5	1500	34·5	533·40	2·3	
6	1600	32·	493·82	2·0	
7	1650	34·65	534·71	2·1	120 lbs. 9 oz.
AVERAGES OF URINE OF A. C.					
	1558	35·35	545·45	2·266	120 lbs. 10 oz.

TABLE X.

Diet of A. G.

	grms.	Water.	Water-free Food.	Albuminates.	Fats.	Carbohydrates.	Salts.
Bread	336·	134·78	202·18	26·95	5·05	165·78	4·38
Butter	228·	1·36	21·32	·06	20·63	...	...
Sugar	478·	1·43	46·37	...	...	46·127	0·239
Eggs	112·	82·32	29·68	15·12	12·992	...	1·12
Milk	425·	381·06	56·94	17·52	16·20	21·9	2·628
Rice	30·9	3·09	27·87	1·54	·247	19·573	·154
Beef	113·4	85·05	28·35	17·01	9·525	...	1·8
Potatoes	113·4	83·916	29·484	1·70	·113	26·435	1·134
Total in Grammes		773·0	442·37	79·90	64·757	279·815	11·455

Water in Tea Infusion in above Diet 1200 C. C.

The Albuminates contain 12·68 grms. = 195·67 grains of Nitrogen.

The Albuminates, Fats and Carbohydrates contain 218·178 grms. = 3366·48 grains of Carbon.

TABLE XI.

Mental Work. Urinary Analyses of A. G.

Period and Condition.	Numb r of Days.	Date.	Quantity.	Density.	Urea.	Grammes. Na Cl	Grammes. PO_5	Weight.
I. As little Mental work as possible.	4	Sept. 25	C. C. 1500	1017	grains. 422·164	...	2·304	
		26	1610	1016	465·074	8·758	2·611	
		27	1300	1022	442·900	10·336	2·595	
		28	1100	...	439·940	7·549	2·365	140 lbs.
		29	2110	...	640·727	11·479	3·780	62·72 kils.
II. Severe Mental work.	4	30	920	1025	347·508	8·274	2·044	$135\frac{1}{2}$ lbs. = 61·59 kils.
		Oct. 1	1880	1020	583·909	13·060	3·128	
		2	1680	1024	460·381	10·124	2·623	
		3	1580	...	520·743	10·844	3·223	
III. Little Mental work.	1	4	1600	1018	391·057	10·177	3·070	...

TABLE XII.

MENTAL WORK. URINARY ANALYSES OF J. W. P.

Period and Condition.	Number of Days.	Date.	Quantity.	Density.	Urea.	Grammes. Na Cl	Grammes. Po_5	Weight.
I. As little Mental work as possible.	4	Sept. 25	1540	1025	grains. 707·156	13·533	3·469	
		26	1775	1021	762·488	12·628	3·635	
		27	1832	1023	746·268	13·800	3·751	165 lbs. = 75· kils.
		28	1750	1024	777·672	15·672	3·584	
II. Mental work daily increasing.	5	29	2170	1020	819·686	16·528	3·333	
		30	1635	1026	799·224	13·00	3·767	164 lbs.
		Oct. 1	1965	1026	844·107	17·762	3·903	163¾ lbs.
		2	1970	1024	787·892	17·029	3·419	...
		3	2365	1021	822·486	19·201	3·511	163½ lbs.
III. As little Mental work as possible.	3	4	1730	1026	807·227	15·928	4·074	...
		5	1575	1026	692·614	15·160	3·064	...
		6	1720	1027	743·108	17·275	...	163¼ lbs. = 74·2 kils.

TABLE XIII.

The Urinary Averages during the periods of Mental Rest and Mental Work.

A. Averages of Urine of A. G.

Period.	Quantity.	Density.	Nitrogen, in Grains.	Grammes. Na Cl	Grammes. Po_5
I.	C. C. 1352	1018	206·5	8·881	3·291
II.	1515	1023	223·129	10·575	2·754
III.	1600	1018	182·493	10·177	3·070
B. Averages of Urine of J. W. P.					
I.	1724	1023	337·584	13·90	3·6
II.	2021	1023	380·150	16·704	3·586
III.	1675	1026	348·9	16·121	3·569

TABLE XIV.

Urinary Analyses under Experiments with Broom Tops.

Medicine.	Numb. of Days.	Date.	Quantity.	Density.	Urea, per Cent.	Urea. Grms.	Urea. Grains.	Calculated Solids. Grms.	Calculated Solids. Grains.	Grammes. Na Cl	Grammes. Po_3	Grammes. So_3
		March	C. C.									
Scoparine	6	14	1940	1020	2·665	51·701	797·849	92·1	1421	7·357	3·277	...
		15	1962	1027	2·767	54·298	837·823	126·6	1953	...	3·515	3·60
		16	2230	1021	2·613	58·286	899·362	111·3	1717	...	3·658	...
		17	2030	1022	2·767	56·180	866·861	106·3	1640	12·1	4·001	3·595
		18	1990	1021	2·408	47·934	739·623	99·3	1532	20·15	3·056	...
		19	1980	1020	2·613	51·752	798·537	94·0	1450	...	3·346	...
Pills of the Alcoholic Extract of Broom Tops	3	20	2276	1019	2·255	51·323	791·926	103·6	1598	16·018	3·612	3·193
		21	2070	1027	2·562	51·171	824·442	134·0	2067	...	3·612	...
		22	1825	1023	2·767	50·506	779·321	99·9	1541	...	3·550	...
Nil		23	1985	1022	2·767	54·934	847·645	103·9	1603	13·807	3·557	3·554
Sparteia	4	24	2275	1020	2·562	58·296	899·520	107·1	1652	14·482	4·076	...
		25	1610	1020	3·382	54·458	840·290	76·5	1180	...	2·984	...
		26	1819	1025	3·075	55·934	863·065	107·8	1663	...	3·158	...
		27	2112	1021	2·562	54·120	835·071	105·4	1626	16·013	3·465	4·101
Decoction Scoparii	6	28	2380	1019	2·255	53·669	828·112	107·3	1655	...	3·655	...
		29	2500	1014	2·050	51·250	790·787	82·6	1274	...	4·096	...
		30	2320	...	...	...	...	...	...	...	...	...
		31	2480	1019	2·255	55·924	862·909	111·8	1725	...	4·422	...
		Apr. 1	2280	1025	2·665	60·762	937·557	136·1	2100	...	3·969	...
		2	2340	1020	2·460	57·564	888·212	111·1	1714	...	3·36	...

TABLE XV.

Urinary Averages under the Experiments with Broom Tops.

Medicine.	Water of Ingesta.	Urine.	Density.	Urea. Grains.	Grammes. Na Cl	Po_5
Scoparine	2858	2022	1022	821·6	13·27	3·475
Extract of B. T.	2858	2058	1023	797·2	16·018	3·591
Nil	2858	1985	1022	847·6	13·807	3·557
Sparteia	2858	1954	1021	859·4	15·20	3·420
Infusum Scoparii	3242	2383	1019	861·5	...	3·90

If the quantity of Infusum Scoparii were deducted from the Quantity of Urine we should have only 1995 C. C. passed daily.

AN ACCOUNT OF THE POST-MORTEM EXAMINATION OF A CASE OF ANEURISM OF THE ABDOMINAL AORTA CURED BY PRESSURE. By Wm. Murray, M.D., *Newcastle-on-Tyne.*

In the year 1864 I communicated to the Medico-Chirurgical Society of London (*Transactions,* 1864), an account of a case of aneurism of the abdominal aorta, for the cure of which I had employed compression of the artery by the rapid method immediately above the tumour. The patient was a man, aged 26, whose occupation as a paviour had compelled him to use a large wooden rammer for driving paving stones into the ground. Between the abdominal aneurism and the free borders of the left ribs there was space enough to permit one blade of a tourniquet to press down on the spine, and on tightening the tourniquet I found that, by a nice adaptation, the pulsation in the aneurism could be completely commanded. The patient was put under chloroform on April 16th and pressure applied for two hours, and a second time on April 19th and the pressure and insensibility were kept up for about five hours. On the next day no pulsation was felt in the aneurism which was hard, resistant, and lessened in size. Neither was pulsation to be felt in the aorta below the tumour, in the iliacs, or in the femoral arteries. On the 23rd pulsation became distinct in the small arteries in the abdominal wall. On the 26th the patient walked about a quarter of a mile. On the 7th July, 1864, the patient obtained work as a 'fitter.' The lower limbs were plump but flabby, the rest of the body well nourished. The tumour was scarcely to be felt, and the aorta, iliacs, and femoral arteries, were quite pulseless.

The patient continued to enjoy good health until the commencement of the year 1870, having during the intervening six years followed a variety of laborious occupations, and having also on several occasions undergone no small amount of privation through the prevailing scarcity of work. During this period the aneurismal swelling gradually disappeared, the termi-

nation of the aorta could be very easily distinguished by its thud against the hand when applied about three inches above the umbilicus, and its course below the thudding point remained absolutely pulseless.

By far the most notable change during this period was the appearance of numerous large pulsating vessels on the front and sides of the abdominal parieties; one on either side of the rectus muscle on the site of the epigastric arteries equalled the femoral artery in size, while those on the upper part and lateral aspects of the abdomen varied from the size of the brachial artery to that of the ulnar. The course of these vessels was, for the most part, extremely tortuous and difficult to trace.

About the commencement of the year 1870 the patient was compelled to resume his old occupation as a paviour, and the strenuous efforts required of him once more brought on violent pain in the epigastrium, which was speedily followed by other symptoms of an aneurism in that region. This latter disease, when fully developed, was found to lie so close to the diaphragm that pressing the aorta above it was out of the question. The aorta below this new aneurism was very carefully examined and found to be perfectly free from pulsation, in fact it was evident that the aorta above the occluded point had given way and become dilated into an aneurism. The usual symptoms of aneurism of the aorta near the cæliac axis were terminated by the sudden death of the patient on June 1st, 1870.

The *post-mortem* examination was conducted by Dr Maclachlan and Mr Davidson, assisted by Mr Johnson, in the presence of Mr Russell, one of the surgeons to the Newcastle Infirmary, and several other gentlemen. To all these gentlemen I am much indebted for the assistance they afforded, but more especially to Dr Maclachlan, who carefully dissected the aneurism after its removal from the body.

On removing the skin from the front of the abdomen a numerous array of tortuous blood-vessels was found ramifying in every direction; the tortuous branches of neighbouring trunks were seen to anastomose directly with each other, and the terminations of the trunks themselves were observed to be continuous with each other. The deep epigastric artery (as large as the axillary) ran up along the outer border of the rectus

muscle, giving off lateral branches; other branches of the epigastric passed outwards and anastomosed with the lower intercostal arteries, these latter being much enlarged and tortuous. One lateral branch of the epigastric given off from its inner side penetrated the umbilicus, and running along the free border of the suspensory ligament of the liver, entered the longitudinal fissure of that organ and anastomosed with a branch of the hepatic artery. The superficial epigastric artery, enlarged and very tortuous, entered a plexus of vessels formed by it and branches of the lower intercostal arteries. The superficial circumflex iliac followed the same course and joined in an anastomosis with the lower intercostals.

After opening the abdomen the superior mesenteric artery, as large as the aorta, was apparent, and the colica media branch enormously enlarged gave off branches of a very large size, which joined the anastomosis of similarly enlarged vessels given off from the colica sinistra branch of the inferior mesenteric. All these anastomosing vessels were as large as crow-quills, even at their points of union with each other. The state of the inferior mesenteric artery was most peculiar, for while giving off these large branches the trunk of the vessel was dwindled to the size of the radial artery, and its coats were thin and flaccid. This wasted state of the vessel was easily accounted for by finding that the vessel entered that part of the aorta which had been occupied by the first aneurism, and which was now a mere fibrous mass. It was evident, therefore, that a very free current had been sent through this anastomosis of the colica media and sinistra, but it must be further noticed that the sigmoid and superior hæmorrhoidal branches of the inferior mesenteric were also very much enlarged, and their branches entered very freely into their network of anastomosis lying on the left of the aorta and between it and the descending colon. The rest of the *visceral* branches of the aorta were in their natural state, but a very large and free union was found on the surface of the iliacus and quadratus muscles, between branches of the last lumbar and ilio-lumbar artery (which in this instance was given off from the common iliac). This anastomosis was also joined by the circumflex ilii and by branches of the upper lumbar arteries, these latter much enlarged.

The circumflexus ilii divided into two branches at the crest of the ilium (one as large as the radial, the other as large as the ulnar); the upper joined the anastomosis of the ilio-lumbar and lower lumbars, the other joined the large terminal branches of the upper lumbar arteries.

Thus, it will be seen—1st, That outside the abdomen the circulation was carried on between the internal mammary and deep epigastrics; the hepatic artery and a branch of the epigastric; the intercostals and deep epigastric; the intercostals and superficial epigastric; the lower intercostals and the superficial circumflex iliac.

Within the abdomen the circulation was carried on between the colica media and the colica sinistra branch of the inferior mesenteric, with its sigmoid and hæmorrhoidal branches; the upper lumbar arteries and the ilio-lumbar; the lower lumbar arteries and the circumflexus ilii.

No anastomoses were found between the visceral and parietal branches of the aorta, except between a branch of the epigastric and the hepatic. In this respect the case differed very materially from the condition met with by Dr Chiene in a case of aneurism of the aorta, where the cœliac axis, superior and inferior mesenteric arteries, were obstructed at their origin, a description of which will be found in the 3rd volume of this *Journal*, p. 65.

As regards the state of the aorta itself, it was observed to be largely dilated up to the new aneurism, which had become "diffuse" before death, and rendered any careful dissection impossible, as the whole of the precincts of the aorta were completely occupied by large coagula and masses of semi-coagulated blood. The aorta below this was completely occluded, and its walls in an atrophied condition.

The importance of the case is greatly enhanced by the *post-mortem* evidence of its reality which has been now obtained. Until this was procured I hesitated to press its claims on the notice of the profession, lest there should have been any error of diagnosis. Now that all fear on that account is removed I would claim for the case great importance, because it involves in itself not one but several facts new to anatomy, physiology, and practical medicine. It establishes,

1st. The possibility of suddenly blocking up the aorta below the renal arteries without injury to the patient.

2nd. It reveals the channels by which blood finds its way to the lower part of the body when the aorta is thus occluded.

3rd. It shows the vast importance of giving chloroform in using pressure for the cure of aneurism.

And 4th. It proves that aneurism can be cured in a few hours by coagulation of blood, while the old method which cured the disease by fibrinous lamination lasted, on an average, five and twenty days.

LIGAMENTOUS ACTION OF THE TRAPEZIUS MUSCLE PATHOLOGICALLY ILLUSTRATED. By Professor CLELAND, *Galway.*

A CASE which has occurred in my hospital practice presents some points of anatomical interest, which may render it not unworthy of notice in this *Journal*. The patient is a young man who was discharged some years ago from the army on account of bad health. He suffers from lupus, which proves very amenable to treatment, but breaks out again after he has left hospital. He was treated in summer for a large ulceration of the right side of the neck, between the borders of the sterno-mastoid and trapezius muscles, precisely opposite the position of the spinal accessory nerve. As the ulcer was simply dressed with permanganate of potash, it was left to the care of a wardsman, who certainly passed the turns of his bandage constantly over the shoulder of the affected side and underneath the other shoulder; but that is not sufficient to account for the deformity found to exist when the patient was able to sit up. When he sits with the vertebral column and sternum perfectly straight, he has yet an appearance of being twisted and bent forward, which is entirely due to loss of the action of the trapezius muscle. The clavicle is fallen downwards and forwards, the shoulder is at a much lower level than the other, and the inner border of the scapula is removed outwards. The position of the whole shoulder-girdle of the right side is one which cannot be imitated on the left; and we have thus a beautiful illustration afforded that the human shoulder when at rest is not merely supported by the sterno-clavicular ligaments, but is hung from the neck by the trapezius muscle. It appears, therefore, that the trapezius affords an example of that ligamentous action of muscles to which I formerly drew attention[1], whereby, through the shortness of their fibres, they limit the amount of movement which otherwise would be allowed by the construction of the skeleton.

It may be noted that in this case, although the shoulder hangs down when at rest, the power of shrugging it by means

[1] In this *Journal*, November 1866.

of the levator anguli scapulæ remains; and although the scapula at rest falls outwards, the power of approaching it to the spine by the rhomboid muscles remains. On account, apparently, of the acromion being thrown into an oblique position, and probably also in part by the action of the sterno-mastoid muscle, the patient can still raise his arm above the level of the position at right angles to the trunk, although that position is the highest to which in normal circumstances the arm can be elevated by the deltoid muscle, and further elevation involves rotation of the scapula. The clavicular portion of the pectoralis major co-operates with the deltoid in raising the arm, standing out rigid as if it formed with it one muscle; a result which certainly I should not have expected, and which I can only explain by taking into consideration that the patient cannot stretch his arm directly outwards, but, when told to do so, twists his spine, and stretches his arm forwards, and that the clavicular part of the pectoralis major has normally the power of elevating the humerus with an inward motion. When the elevated arm of

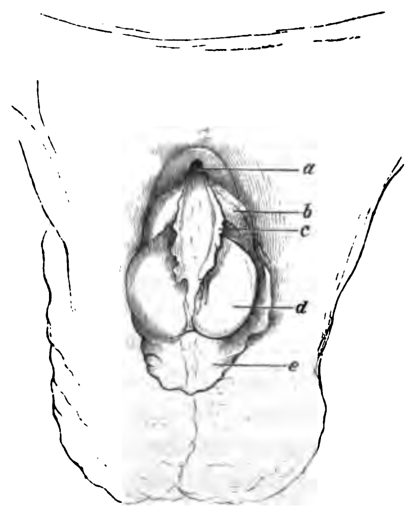
the patient is laid hold of, and drawn back so as to point directly outwards from the body, it springs forwards again as soon as the pressure is removed.

NOTE. About a year after the writing of these remarks the patient died of pharyngeal disease and scirrhosis of the liver; and it was found to be really the case that the spinal accessory nerve was involved in the cicatrix of the ulcer referred to, and could not be traced further. The upper part of the trapezius had entirely disappeared, while the rest of the muscle was extremely thin, but of a red colour, having been probably supplied by undivided branches of nerve.

A CASE OF EPISPADIAS, WITH REMARKS. By Professor CLELAND, *Galway*.

ALTHOUGH there have now been a number of cases of epispadias recorded, and references to the cases described by Eastlake[1], Bergh[2], and Bryant[3], have been given by Professor Turner in previous numbers of this *Journal*, the malformation is sufficiently rare to make a drawing of a well-marked example still interesting.

In the accompanying sketch the penis is in the position in



a, meatus urinarius; *b*, integument of body of penis; *c*, fringe along the margin of the urethral groove; *d*, glans; *e*, pendulous prepuce.

[1] *Med. Times and Gaz.* July 20, 1867.
[2] *Virchow's Archiv*, XLI. p. 305.
[3] *Guy's Hospital Reports*, p. 419.

which it could be placed by pressing the glans slightly downwards with a finger; only, for the sake of clearness, the finger is not represented. The patient is a well developed boy seven months old, with the testes completely descended.

The fringe on each side of the urethral groove, separated by a sulcus from the glans externally, is certainly the margin of a corpus spongiosum which has failed to meet over the urethra with its fellow of the opposite side. The example of epispadias in the adult dissected by Bergh demonstrates that point; for although in Bergh's case there appears to have been little or no fringe, the transverse section showed that the urethral groove lay on the dorsum of a well marked corpus spongiosum, above and between the corpora cavernosa. In this case, as in the one figured by Bergh, well-marked foramina of Morgagni were seen in the floor of the urethral groove. The child, as far as can be as yet judged, has full power of retaining its urine, and as it happened to urinate while the sketch was being made, it was observed to have the power of projecting the very full stream a little distance from the body.

There is still considerable difficulty in the way of understanding the development of malformations of this description. W. Frolik[1] casts aside the hypothesis of rupture, and imputes both simple epispadias and ectopia vesicæ to arrest of development: and in this Professor Humphry appears to agree with him. But epispadias, whether with or without ectopia vesicæ, is, when the glans is developed, something else than a failure of lateral parts to meet in the middle line. In normal circumstances the earliest appearance of the genitals would seem to be as a papilla in front of the cloaca, with lateral folds passing down by the sides of that orifice; and this papilla makes its appearance after the base of the allantois has been covered in by superficial structures. In simple epispadias there is an infra-pubic opening superior to the genital papilla, instead of patency of the original passage below it; and in the example dissected by Bergh it was demonstrated that the pubic symphysis was normal. In cases of ectopia vesicæ with development of the glans, there is the same obliteration of a primary

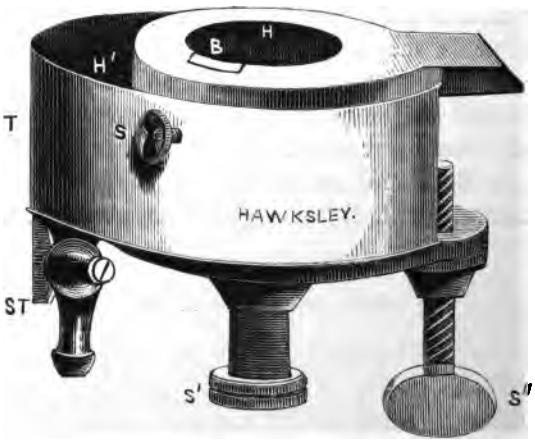
[1] Article *Teratology* in *Cyclopædia of Anatomy and Physiology.*

opening beneath the penis, with a larger abnormal opening above it, and there may be perfect development of the glans, although, as in the case the dissection of which has been recorded by Professor Humphry in this *Journal*, III. 81, the pubic arch may remain imperfect. Now, it is not easy to see how an opening above the genital papilla can exist either in the form of simple epispadias or ectopia vesicæ without early rupture of the anterior wall of the uro-genital sinus or of the allantois, seeing that the allantois is not developed in lateral parts, but as a mesial structure. Such a rupture might arise in consequence of obliteration of the normal genito-urinary orifice by tightness of the septum which, at a period later than the appearance of the genital papilla, cuts off the sinus uro-genitalis from the rectum. The position of the corpus spongiosum above the corpora cavernosa becomes explicable by assuming that the corpus spongiosum is developed from behind forwards, and is fundamentally neither above nor below the corpora cavernosa, but an erectile development between the crura, and more closely embracing the genito-urinary canal. Such cases as that described by Luschka[1], in which a small canal with a detached portion of prostatic gland was found on the dorsum of an otherwise normal penis, may be explained by the supposition that a temporary stricture had occurred, such as I have supposed, in the development of the perinæal band in early fœtal life, but that it had been overcome, and that the abnormal passage, ceasing to be developed as the normal one grew larger, had dragged forwards with it some glandular structure from the prostate.

[1] *Virchow's Archiv.* XLIII. p. 592.

ON SOME IMPROVEMENTS IN THE MODE OF MAKING SECTIONS OF TISSUES FOR MICROSCOPIC OBSERVATION. By WILLIAM RUTHERFORD, M.D., F.R.S.E. *Professor of Physiology, King's College, London.*

MR STIRLING, the Assistant Curator of the Anatomical Museum in the University of Edinburgh, deserves the thanks of all histologists for the excellent Section Machine invented by him some years ago, and described by him in this *Journal* (p. 230, May, 1870). I have instructed my students how to use this machine for several years, and I can therefore bear testimony to its excellent capabilities. Three or four years ago I modified the machine in a manner which rendered it more extensively useful, and recently I have added to it another arrangement which I deem so important that I think it worthy of being brought under the notice of all histologists.



Machine used in making sections of vegetable and animal tissues. Half its real size. (For description, see text.)

Mr Stirling's machine consists essentially of a cylindrical tubular cavity (*H*), the bottom of which is closed by a brass

disc or washer which can be raised or depressed by means of a screw (S'). The upper part of the cavity is open and is surrounded by a perfectly flat brass plate upon which the knife glides when the section is being made. The machine can be clamped to a table by means of a screw (S''). The tissues to be cut are placed in the hole H. If their shape be irregular they should be imbedded in some such substance as paraffine; ordinary paraffine is too hard for most purposes. By the following formula, suggested by Dr Ferrier, Demonstrator of Practical Physiology in King's College, one can readily make a fixing material which, in general, answers admirably.

Solid paraffine 5 parts.
Spermaceti 2 parts.
Axunge 1 part.

By varying the amount of axunge one may obtain a substance of any desirable consistence which melts at a low temperature —is easily cut—does not stick to a wetted knife, and which readily separates from the tissue which may be imbedded in it. This substance should be kept in a covered tin vessel, such as a small coffee-pot; in which it may be protected from dust, readily melted, and from which it can be easily poured. This fixing agent answers best for most things. If, however, the body to be cut have a tolerably regular outline, it may, as Mr Stirling recommends, be imbedded in a carrot, or in a *white* or *yellow* turnip—from which all sand has been removed by careful washing. With this view a cylindrical piece of the vegetable is cut by means of a cork borer—the calibre of which is equal to that of the cavity in the section machine. A suitable hole is then carved in this vegetable cylinder by means of a cork-borer of convenient size, or by means of a knife, and into this hole one places the tissue to be cut, and the entire arrangement is then pushed into the cavity in the machine, and, if need be, is wedged therein by means of little strips of carrot or turnip. Undoubtedly the knife cuts the vegetable more readily than the paraffine support.

The thickness of the sections is of course regulated by the extent to which the screw S' is turned. In Mr Stirling's machine this is done by guess—a serious inconvenience to

beginners, indeed to anyone who desires to make sections of uniform thickness. This lack of precision is of course very readily supplied by placing an indicator in connection with the screw, so that one may give an $\frac{1}{8}$, or $\frac{1}{4}$, or $\frac{1}{2}$, or a whole turn of the screw, and so on. The botanists object to this machine on the score that it is troublesome to use it for cutting a hard thing, like a piece of wood, unless this be made to fit the hole (*H*) or be imbedded in something or other. This want I have supplied by having a small segment (*B*) cut out of the wall of the cavity *H*. The breadth of this segment is about $\frac{1}{8}$ the circumference of the cavity, and its depth is about $\frac{1}{2}$ an inch. The segment has a screw (*S*) and two guiding rods attached to its outer surface. By means of the screw it can be pushed forwards to any distance towards the opposite wall or retracted, as represented in the figure. By means of this clamp hard things can be at once fixed and cut. When this clamp is used an oval rod of wood or brass of smaller diameter than the structure to be cut must be placed below it, so that it may rest on the top of the screw S' and serve to communicate the movement of this screw to that of the tissue to be cut. This arrangement is necessary because the clamp projects over the top of the screw S', and the tissue could not be moved upwards by this screw unless it projected below the clamp.

The last and most important modification which I have made on this apparatus supplies a want which every advanced histologist must have experienced. All soft tissues require to be hardened ere they can be cut in a machine of this sort. It is in many cases extremely inconvenient to wait from two to six weeks until a tissue be hardened in chromic acid, &c. The mode of hardening tissues by freezing is well known. Most beautiful sections of frozen lung, liver, kidney, muscle, skin, brain, &c., may be made by an ordinary razor. Sections may be made in this way much finer and larger than they can by means of Valentin's knife from the unhardened tissue. Moreover this mode of hardening tissues possesses in some cases, and for some purposes, many advantages over that in which hardening fluids are employed. I have, I confess, hitherto adopted the freezing process but little, owing to lack of any convenient method for cutting the frozen tissue. It is customary to put the

tissue into a metal crucible surrounded by a freezing mixture, and then to make sections as best one can. The method is clumsy and inconvenient. I have succeeded in rendering it simple and satisfactory by the following modification of the above-mentioned machine. I removed the wood which surrounded the brass tube (around *H*) in Mr Stirling's machine, and enclosed the space previously occupied by the wood by a piece of thin brass (*T*). The upper and lower edges of this brass are soldered everywhere, excepting at the left side, to the borders of the horizontal brass plates at the top and bottom of the instrument. A freezing mixture, consisting of salt and pounded ice, is put into the chamber (*H'*) enclosed by *T*. This is readily introduced at the left side, where, with this view, the box is made to project. A stopcock (*ST*) permits the fluid resulting from the melting of the ice to flow away when required.

When I want to freeze a tissue I put it as far down into the hole *H* as it can go, and I pour into the hole the ordinary neutral salt solution (0·75% solution of common salt in water). I then fill the box *H'* with the freezing mixture, and cover the whole machine with flannel or cotton wadding to prevent as far as possible the admission of heat. If this amount of freezing mixture do not suffice, the stopcock is turned to allow the fluid to flow out of *H'*, and more freezing mixture is introduced. The entire contents of the cavity *H* are generally rendered quite solid in from ten to fifteen minutes. If the tissue to be cut be very small, it should be held in the fluid within *H* against the wall. On this a layer of ice soon forms sufficiently thick to enclose the object. Once the tissue is frozen it is of course very easy to keep it in this condition. The next thing to be done is to cut the frozen tissue and the ice in which it is enclosed. To do this satisfactorily baffled me for some little time. When the ice is perfectly hard and crystalline it splinters, and on this account the section is much broken, and I need scarcely say the face of the knife suffers sadly. I find, however, that by breathing on the knife and the ice while the section is being made, or by dipping the knife into water gently warmed, or by brushing the above-mentioned salt solution at a temperature of about 65° Fah. over the surface of the ice and tissue to be cut, the crystalline condition of the

ice is undone, and beautiful sections may be made if the knife be passed through it with a very rapid sawing movement. Of course the ice around the section soon melts, and therefore no trouble is required to remove the fixing agent, as in the case of paraffine, carrot, &c. The capabilities of this arrangement will be readily appreciated from the following experiment. I killed a medium-sized rabbit, and immediately removed a portion of a lung, liver, intestine, muscle, and a whole kidney. I rapidly washed them in salt solution (vide supra), and put them, hot as they were, at once into the machine and covered them with salt solution. I put the freezing mixture into the box and covered the whole with cotton wadding. The box *H′* required to be about half refilled with the freezing mixture ere the tissues were completely frozen. To freeze them thoroughly required sixteen minutes. I then made sections, as fine as any one could possibly desire, of all the different tissues at once, picked them off the knife with a camel-hair pencil and put them into separate vessels for examination. Perhaps some supporting fluid other than salt solution will be suggested by future research. I have not as yet been able to find a better. Perhaps, too, other modifications of the machine may be suggested, but for the present I am unable to think of anything that could be done to render the apparatus more perfect than it now is. For making the sections I use a razor, a circular amputating knife, or a knife with a long broad blade ground flat upon one side.

The machine as above described is made by Mr Hawksley, Blenheim St., New Bond St., W.

ON THE RELATIVE EXCITABILITY OF DIFFERENT PARTS OF THE TRUNK OF A SPINAL NERVE.

By WM. RUTHERFORD, M.D., F.R.S.E., *Professor of Physiology, King's College, London.*

As is well known, Pflüger some years ago showed that if the sciatic nerve of the frog be stimulated at one time near the gastrocnemius and at another time at a distance from the muscle, the contraction which takes place is more powerful in the latter than it is in the former case, even although the irritant be of the same strength in both instances.

The manner in which this experiment is performed will be readily understood with the aid of the following diagram.

Fig. 1.

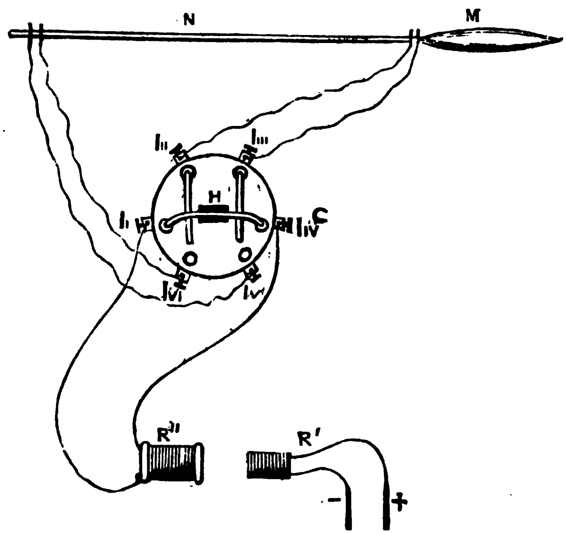


Fig. 1. Showing the arrangements for performing Pflüger's experiment described in the text. *N* Sciatic Nerve. *M* Gastrocnemius of frog. *R'* and *R''* primary and secondary coils of Du Bois Reymond's Induction machine. *C* Pohl's commutator[1], with wires connecting it to the induction machine and to the nerve.

[1] For an account of the mechanism of Pohl's commutator the reader is referred to my Lectures on Experimental Physiology in the *Lancet*, No. VI. Vol. I. 1871.

The sciatic nerve of the frog is dissected out in its entire length, and is divided as near the spinal column as possible. The femur and muscles of the thigh are divided, and the leg, with its nerve, is removed. In cleaning the nerve it is important to remember that it must not be stretched or scratched, otherwise the experiment may completely fail. Every care must be taken to injure the trunk of the nerve as little as possible. The nerve must not be allowed to dry, else failure is inevitable. I find it best to perform the experiment the moment the nerve is cut out. If one be compelled to wait for more than three or four minutes the nerve should be pencilled with saliva, allowed to cool in a watch glass. The nerve (*N*) is laid upon two pairs of clean copper wires, one pair placed near the gastrocnemius (*M*), the other pair placed near the free end of the nerve. The wires are joined to a Pohl's commutator (*C*), and this is connected by means of another pair of wires with the secondary coil (*R''*) of Du Bois Reymond's Induction machine. The primary coil of this (*R'*) is joined to a battery. When the bridge of the commutator (*H*) is in the position indicated in the figure the faradic currents pass through the nerve near the muscle by the wires joined to $1_{\prime\prime}$ and $1_{\prime\prime\prime}$. When the bridge is turned so as to make it lean to the side of the commutator which is lowest in the diagram, the currents no longer pass through the above-mentioned wires, but through those touching the nerve near its free end. Before the experiment is begun, the coils (*R'* and *R''*) of the induction apparatus are separated from each other to a distance of about 20 inches. The bridge of the commutator is placed so that the nerve will be stimulated near the muscle. The induction machine is then thrown into operation. The muscle will most probably remain motionless, because the induced currents are not powerful enough to affect the nerve. To render them sufficiently strong, the secondary coil (*R''*) is very slowly pushed towards the primary coil (*R'*), and this movement is arrested the moment the toes are thrown into a slight tremor, indicating feeble contraction of the muscles of the leg. Without arresting, or in any way altering the action of the induction machine, the bridge of the commutator is turned, so that the nerve will be irritated at a distance from, instead of near to, the muscle. If

the experiment succeed, it will be observed that the contraction of the muscles is now very decidedly exaggerated; and this, although the irritant for all practical purposes remains of uniform strength. We may vary the experiment, and begin by finding out the feeblest currents necessary to throw the muscle into contraction when applied to the nerve near its free end, and then cause this current to operate on the nerve near to the muscle. Most probably, in the latter case, the leg will come to rest, the irritant not being powerful enough to affect the nerve in the latter situation.

This, then, is the experiment for which we are indebted to the distinguished Bonn physiologist. How are the results to be explained? According to Pflüger the experiment shows that the passage of nerve force along a nerve resembles the rush of an avalanche over a precipice. It gathers force as it descends. If it were true that the nerve influence passes through a nerve in this avalanche-like (lawinartig) manner it would be a fact of great importance in nerve physics, for it would show that the longer the nerve through which the nerve energy must pass, the easier is it to produce a result in the tissues at the nerve terminations; in other words, the longer the nerve the feebler need be the stimulus applied to it in order to produce a given result. I have long been in the habit of pointing out to my class that Pflüger's explanation cannot be accepted until it be proved that the result of the experiment is not simply due to the excitability of the nerve being less near than it is at a distance from the muscle. Rosenthal long ago discovered the fact that if a nerve be tied or divided, its excitability is for some time increased. This increase is most marked near the point of injury. This fact I have repeatedly verified. Pflüger cut the sciatic nerve, as I have described, and found that an irritant applied near the cut end produced a greater effect than when applied at a distance from it. This might simply be due to the greater irritability of the nerve near the seat of injury; and, moreover, the greater irritability of the nerve at this point might be owing to its being nearer the injured part. I have always regarded this explanation as more probable than Pflüger's. Experiments recently performed by me serve, however, to materially advance our knowledge

regarding this interesting question, and, indeed, to place the matter in a new light.

I performed a number of experiments on frogs in the following way. I exposed and carefully dissected out one sciatic nerve. I tied the sciatic artery, and divided the muscles and bones of the thigh. I hung the frog up by a pin thrust through the point of the nose, and I fixed the femur in a pair of forceps in order to support the nerve and place it for convenience in a somewhat horizontal position.

Fig. 2.

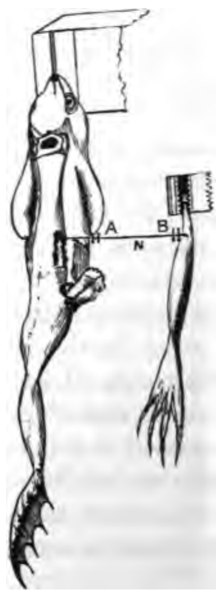


Fig. 2. Frog with right leg partially amputated. Right sciatic nerve *N*. *A* and *B* two pairs of wires for stimulating the nerve. (For further explanations see text.)

In order to irritate the nerve I placed two pairs of wires, as indicated in Fig. 1, under the nerve, taking care that the wires touched nothing but the nerve. In order to render the insulation of the whole more complete, I placed the stands supporting the frog and its detached leg upon a sheet of dry gutta percha. I did not divide the nerve, because I wanted to avoid

the increase of its excitability which this would have occasioned. In two frogs the power of voluntary motion remained, while in five this was paralysed. In one of these a grain and a half of chloral, in the other four, twenty minims of tincture of opium were injected under the skin of the throat. In every case I sought for the feeblest current which was just sufficient to cause the toes of the right foot to tremble when brought to bear upon the nerve at *B* (Fig. 2). On applying this irritant to the point *A*, a much more decided contraction of the muscles of the leg invariably followed. Hence I found here just Pflüger's result, which I have described above. *But the motor nerve fibres implicated had not been divided. Therefore it cannot be held that lesion of the nerve was the cause of the irritability being greater at A than at B.* It occurred to me that Pflüger's avalanche hypothesis might be put to the test in the following way. He experimented only on motor nerves. I resolved to do so upon afferent nerves (excito-motor). Were Pflüger right, then, we ought to find that an irritant of uniform strength produces stronger reflex action in the left leg and arms when applied at *B* than when applied at *A*; for, in the former case, the influence has a longer tract of nerve through which to pass. (Vide supra.) The result, however, was precisely the reverse of what Pflüger's hypothesis led one to expect. The irritant produced the most powerful reflex action *not* when applied at *B* but when applied at *A*. I am, therefore, obliged to say that Professor Pflüger's explanation cannot in my opinion be entertained.

It may be well that I should indicate more precisely the results which followed the stimulation of the nerve in these cases. As above-mentioned, I found out first of all the feeblest electrical stimulus strong enough to cause slight contractions of the detached leg when applied to the nerve at *B*. This stimulus almost never caused the arms or left leg of the frog to move; in short it gave rise to no evidence either of pain or reflex action in the arms or muscles of the opposite leg at any rate. When the same irritant was applied at *A*, the contractions of the detached leg were decidedly increased, the left leg was often drawn up, and sometimes the arms were moved as well. It sometimes happened that in order to produce reflex

action in the arms and left leg by stimulating the nerve at *A*, the irritant required to be stronger than that which sufficed to throw into contraction the muscles of the detached leg when applied to the nerve in this position. In order to increase the excitability of the afferent nerve fibres, I—after making the above observations—divided the nerve between the detached leg and the part over the wires at *B*. This decidedly increased the excitability of the afferent fibres in the part of the nerve at *B*, but in only one instance (Obs. 9, Experiment iv.) did it seem to render them as irritable at *B* as at *A*.

In four other experiments I divided the spinal cord in the cervical region with a view to get rid of sensation and voluntary motion. The results of the experiments in these cases only indicated what the others did, to wit, that both afferent and efferent nerve fibres were more irritable at the point *A* than at the point *B*. Probably in these four cases the division of the spinal cord must have increased its irritability; and, seeing that the portion of the sciatic nerve *A* was nearer to the injured portion of cord than the portion at *B*, it could be held that in these cases the difference between the excitability at *A* and *B* might be due to this lesion of the cord. But we have seen that the same facts were observed in those cases where the cord had not been divided. In these four cases, too, division of the nerve between the wires at *B* and the detached leg did not raise the excitability of the afferent fibres in the part of the nerve at *B* so high as it was in those fibres in the part at *A*. (See Obs. 13, Exp. viii.) This is what one would, *à priori*, have anticipated from the fact, that such a section was generally unable to do this even when the spinal cord had not been divided, and its excitability, together with that of the nerve at *A*, not therefore abnormally exalted.

CONCLUSIONS.

These experiments appear to me to show 1st, that Pflüger's avalanche hypothesis alluded to above is untenable; 2nd, *That both the afferent and the efferent fibres (at least the excito-motor and the motor fibres) of the trunk of a spinal nerve are more*

excitable near the spine than at a distance from it. Although my experiments do not *prove* it, yet nevertheless we may safely substitute the words *nerves centres* for *spine;* and we may therefore say, *That the excitability of any point in the trunk of a spinal nerve is inversely as the distance of that point from the nerve centre.* I say *trunk* advisedly; for I know nothing regarding the excitability of the fibres near the peripheral terminations of the nerve.

This new law leads to novel and interesting questions regarding the influence of nerve centres upon nerve fibres. What can be the cause of the greater excitability of the fibre near the nerve centre or cell? Is it a difference in the blood supply? I think not, because I found that removal of the heart previous to exposure of the nerve, in the manner indicated above, made no apparent difference in the result. A direct action of the nerve cell upon the nerve fibre is probably the cause. Does every nerve cell exert this action upon its fibres? or have only some cells this power? Have these cells a trophic function? that is, do they affect the nutrition of the nerve,—or do they merely exert upon the molecules of the nerve fibrils an influence which renders them more excitable,—the power of this influence being inversely as the distance of the molecules from the cell? I am unable to answer these questions.

It will be readily understood why I have said that a spinal nerve is most irritable near its *nerve centres,* instead of saying, near the *spinal cord.* One has only to remember the ascertained trophic influence exerted by the intervertebral ganglia upon the posterior roots of the spinal nerves in order to perceive that, with regard to this point, these ganglia must not be overlooked.

NOTE. Any one may readily perform the experiments which I have described; but I would remind him that unless he carefully avoid stretching, scratching, or other laceration of the nerve trunk, and unless he take care lest the nerve become dry, he will most probably arrive at results which it will sorely puzzle him to reconcile with those which I have given.

EXPERIMENTS.

Du Bois Reymond's induction apparatus with one Smee's cell used as the irritant in all the experiments.

No. of Experiment.	No. of Observation.	Time.	Distance in millimetres of primary from secondary coil of induction machine[1].	The portion of nerve stimulated.	Results and Remarks.
I.					A strong frog prepared as indicated in Fig. 2. No narcotic given. Nerve moistened with saliva.
	1	10.30′	350	B (see Fig. 2).	Slight contractions in muscles of right leg (see Fig. 2). No movement of left leg or arms.
	2	10.30′.15″	350	A.	Movements of right leg much intensified. Movements (reflex or voluntary) of left leg and arms.
	3	10.31′.4″			Obs. 1 and 2 repeated with similar results.
	4	10.32′.14″	200	B.	Tetanus of right leg. Left leg drawn up. This was the weakest current which caused voluntary or reflex movements of left leg when applied at *B*.
	5	10.32′.50″	200	A.	Tetanus of right leg more powerful. Violent convulsions of the whole body of the animal.
	6	10.33′.11″			Nerve divided between *B* and gastrocnemius.
	7	10.33′.27″	254	B.	This was the weakest current which *now* sufficed to cause movements of the left leg when applied at *B*. The section had therefore increased the excitability of the nerve at this point. But the excitability had not thereby been made equal to that of the nerve at *A*, for although the distance between the coils was increased to 350 mm. movements of the left leg and arms ensued when the nerve was stimulated at *A*.
II.					A repetition of Exp. I. with similar results. In this case however the heart was removed previous to the exposure of the nerve in order to stop the circulation through the nerve centre. *See Conclusions.*
III.					A large strong frog.
	1	4.4′			$\frac{1}{4}$ grain chloral injected under skin of throat.
		4.6′			$\frac{1}{8}$ ditto ditto
		4.8′			$\frac{1}{8}$ ditto ditto
		4.15′			$\frac{1}{8}$ ditto ditto
		4.16′.30″			Animal now completely narcotised. Right sciatic nerve prepared as in Exp. I.
	2	4.19′.5″	340	B.	Slight twitching of right toes. No reflex action in arms or left leg.

[1] The greater the distance the weaker is the induced current.

No. of Experiment.	No. of Observation.	Time.	Distance in millimetres of primary from secondary coil of induction machine.	The portion of nerve stimulated.	Results and Remarks.
	3	4.19'.15"	340	A.	Much more powerful contractions of right leg. No reflex action in arms or left leg.
	4	4.20'.2"			Obs. 2 and 3 repeated with same results.
	5	4.22'.8"	250	A.	Decided reflex action in left leg. Tetanus of right leg.
	6	4.22'.15"	250	B.	No reflex action in left leg. Less marked contraction of right leg.
	7	4.24'.29"	130	B.	The weakest current which sufficed to produce reflex action when applied at *B*.
IV.					A medium sized frog.
	1	1.30'			20 minims Tincturæ Opii injected under skin of lower jaw. Animal completely narcotised thereby. Right sciatic nerve prepared as before.
	2	1.45'	350	B.	Slight twitching of toes of right foot. No reflex action in arms or left leg.
	3	1.45'.18"	350	A.	Tetanus of right leg. Powerful reflex action in arms and left leg.
	4	1.46'.45"	350		Obs. 2 and 3 repeated with similar results.
	5	1.47'.52"	350	A point between A and B.	The wires at *A* were shifted to a point midway between *A* and *B*. The effect of the irritant on the muscles of the right leg was not so marked as when the nerve was stimulated at *A*. No reflex action was observed in left leg.
	6	1.49'.35"	350	A.	Wires were brought back to *A*. Reflex action was produced as in Observation 3.
	7	1.50'.40"	350	B.	No reflex action.
	8	1.51'			Nerve divided between *B* and gastrocnemius.
	9	1.51'.20"	350	B.	Reflex action just as when *A* was excited. This observation was repeated with similar results. The parts of the excito-motor fibres at *B* seemed now to be as irritable as those at *A*.
V. VI. VII.					Experiments similar to No. IV. Results similar excepting that division of the nerve between the gastrocnemius and *B* did not render the excito-motor fibres in *B* so irritable as those in *A*.
VIII.					A medium sized frog. Spinal cord divided in cervical region. Right sciatic nerve prepared as before.
	1	1.48'	330	B.	Slight contraction of right leg.
	2	1.48'.18"	330	A.	More decided contraction of right leg. No reflex action in left leg.
	3	1.49'	320	A.	Stronger contraction of right leg than in Obs. 2. Slight reflex action in left leg.
	4	1.49'.15"	320	B.	No reflex action in left leg. Feebler action of right leg.
	5	1.51'	270	B.	Tetanus of right leg. No reflex action of arms or left leg.

No. of Experiment.	No. of Observation.	Time.	Distance in millimetres of primary from secondary coil of induction machine.	The portion of nerve stimulated.	Results and Remarks.
	6	1.51′.40″	200	B.	Stronger tetanus of right leg. Distinct reflex action in arms and left leg. This was the weakest current which called forth reflex action in left leg when applied to nerve at *B*.
	7	1.52′.18″	310	A.	The weakest current that now called forth reflex action in left leg.
	8	1.52′.26″	310	B.	No reflex action in left leg.
	9	1.53′			Nerve divided between *B* and the gastrocnemius.
	10	1.53′.12″	310	B.	No reflex action in left leg.
	11	1.53′.20″	310	A.	Distinct reflex action in left leg.
	12	1.54′.6″	310		Obs. 10 and 11 repeated. Similar results.
	13	1.56′	245	B.	Reflex action produced. Therefore the section of the nerve had not rendered the excito-motor fibres at *B* so excitable as those at *A*.
IX. X. XI.					Experiments similar to No. VIII. Results similar.

NOTICE OF A CASE OF PECULIAR MALFORMATION OF THE HEART AND GREAT ARTERIES. By A. H. F. Cameron, M.R.C.S., L.R.C.P.

A MALE infant, which lived for not more than between two and three days after its birth, presented on the first day no peculiarity in appearance, but, on the second day, a very faint purple tinge of the skin was observed, though the warmth of the body seemed to be normal. A *post-mortem* examination, made unfortunately in a bad light and under other difficulties, revealed a very peculiar and unusual malformation of the heart and great arteries.

The two ventricles freely communicated with each other through a large opening at the base of the interventricular septum. The two auricles formed practically but a single cavity, as the septum was represented by a mere fold. The auriculo-ventricular valves on the right side were feebly developed; on the left normal. From the base of the left ventricle arose the ascending aorta, which passed almost directly upwards in front of the trachea, and terminated by dividing into two branches, the right branch, somewhat the larger, was the arteria innominata, the left might either be a left innominate, or only the left common carotid. The aorta had only two valves at its cardiac orifice.

From the base of the right ventricle arose a much larger vessel, which inclined upwards and to the left, and was prolonged into the descending aorta. From this large artery arose two branches: the one, which bifurcated behind the ascending aorta, was apparently the right pulmonary artery, the other was in all probability the left pulmonary.

I am unable to speak with certainty of the origin of the left subclavian, as the examination was conducted under very disadvantageous circumstances. It is possible that the left branch of the ascending aorta might have been an innominate, in which case the subclavian would have proceeded from it; or the left subclavian may have arisen close to the spot where the great artery from the right ventricle became continuous

with the descending aorta. The two venæ cavæ were normal and there were four pulmonary veins.

Cases of new-born children in which the two sides of the heart have communicated freely with each other, through imperfect formation of the intermediate septa, have so frequently been recorded that I should not have thought of recording this case, had the cardiac malformation not been complicated with the very unusual defect in the development of the great arteries I have just described.

The case on record which presents the closest correspondence to the above, is one described by Dr Greig in the *Edinburgh Monthly Journal of Medical Science*, July 1852. This case was a fœtus about the ninth month. The two ventricles communicated by a large aperture at the base of the septum. The interauricular foramen ovale was normal, and the valve fully formed. The ascending aorta terminated in three equal branches, right subclavian, and right and left common carotid arteries. The pulmonary artery, nearly twice the size of the ascending aorta, gave off its right and left branches to the lungs; a quarter of an inch further on, the artery gave origin to the left subclavian, beyond which, much diminished in size, it was continued on as the descending aorta.

In a memoir on the Irregularities of the Great Arteries, *British and Foreign Medico-Chi. Review*, July and October, 1862, Professor Turner not only gives an analysis of the cases which had up to that time been recorded, but illustrates their mode of origin by the variations which may occur, either through increased growth or complete atrophy, in the development of the vascular arches and aortic roots in the early embryo. Commenting on Dr Greig's case, Professor Turner offers an explanation, which, as he has kindly pointed out to me, applies equally to the case described in this communication. Atrophy of the fourth left vascular arch had occurred at an early period of embryonic life, in consequence of which the transverse part of the arch of the aorta had disappeared, and the ascending aorta was no longer continuous with the descending thoracic aorta. The blood therefore had to pass to the lower half of the body along that portion of the fifth left vascular arch, which is called the ductus arteriosus, so that the

pulmonary artery and its arterial duct were much larger vessels than the ascending aorta.

It is interesting to note that although the two sides of the heart freely communicated with each other, yet the child during its short life exhibited little or no blue discolouration or diminished temperature.

NOTE ON A PECULIAR ORIGIN OF THE RIGHT SUBCLAVIAN ARTERY. By S. MESSENGER BRADLEY, F.R.C.S., *Lecturer on Human and Comparative Anatomy, Royal School of Medicine and Surgery, Manchester.*

AMONGST the peculiarities met with in the dissecting-room of the Manchester School of Medicine and Surgery during the Session 1870—71[1], there was an instance of the right subcla-

[1] This abnormality existed in the last subject but one which was distributed during the session 1870–71, and it is a curious fact that the next and last subject was affected with a somewhat similar peculiarity. These are the only instances of any peculiarity in the primary branches of the aorta observed in the dissecting-room of the Manchester School for the last five years, during which time I have distributed 120 subjects for dissection. In the second case referred to, both carotid arteries were given off from a short single trunk, and the subclavian arteries arose one behind the other from a point of the aorta further to the left. In this case, as in the one noted above, the right vessel was the more posterior of the two, but did not, as in the former instance, pass between the trachea and œsophagus, but behind them both. Both subjects were females.

There is in the museum of the Manchester School a unique and most remarkable case of abnormality of the aorta itself. The arch is of the ordinary size and in the usual position. The primary branches are given off in the normal manner, with the trifling exception that the left vertebral artery springs directly from the aortic arch. Immediately below the origin of the left subclavian artery, and precisely on a level with the ductus arteriosus, which is rather larger than usual, the aorta suddenly contracts, *terminates* in fact in a blind pouch; on the inferior aspect of this constriction, and connected with it by continuity of structure, the aorta recommences, so to speak, and almost directly regains its ordinary calibre. *There is absolutely no communication between the two portions of aorta which are separated by the constriction*, and the appearance may be correctly conveyed to the reader by the similitude of a sausage with a string tightly tied around its middle. Half an inch beyond the commencement of the descending aorta, that is beyond the constriction, the first intercostal arteries are given off; they are as large as average common carotid arteries, and anastomose with branches almost as large as themselves, derived from the internal mammary vessels of either side. All the intercostal arteries are large and tortuous, and they all anastomose very freely with branches of the internal mammaries, but the principal channel of the collateral circulation is the double one which exists between the two first aortic intercostals and the two internal

vian artery being given off directly from the aortic arch. In this case it sprang from the extreme left of the transverse arch on its posterior aspect, and passed upwards and to the right, running between the œsophagus and trachea to continue down the arm in the usual manner. Most of the peculiarities met with in the human aorta and its branches are representatives of the normal condition of the vascular system in other vertebrates. Thus, *e.g.*, cases are recorded of the aorta arching over the root of the right lung, as is the case in the class of Aves: again, all the primary aortic branches have been seen to spring from a single trunk, and such we know is the condition most usually met with amongst the ruminating Ungulata: more frequently, two Brachio-cephalic arteries have been observed to arise from the aortic arch, which is the ordinary mode of division in the vascular system of the Hedgehog, Mole, and Bat. On the other hand, the peculiarity which is here noted does not seem to conform to the normal anatomy of the aorta of any other animal, nor is it explicable by any reference to embryology.

As generally happens where a single striking abnormality is noted, there were many instances of vascular and muscular peculiarities in the subject here referred to. Amongst the most important of them, I noted that the left facial artery terminated in the inferior coronary, while the right was of normal size and arrangement. The left ulnar artery did not form a superficial palmar arch, but, continuing straight along the inner side of the hand, terminated in a single digital branch which was distributed to the ulnar side of the little finger. There was an accessory pudic artery on both sides. There was no posterior tibial artery on the right side, its place being supplied by a large peroneal artery.

A double *Pronator Quadratus*, which existed on the right side, was the most interesting myological peculiarity.

mammary arteries. Mr Searson, the Curator of the Museum, suggests, as an explanation of this abnormality, that the lower wall of the ductus arteriosus was during embryological development continued quite across the aortic arch, so forming a complete septum between the arch and the descending thoracic aorta.

REMARKS ON A YOUNG AÏNO CRANIUM[1]. By JOHN KENNEDY, M.D. (*Edinb.*), *Elie, Fife.*

SOME time ago I had the opportunity of examining a number of skulls from Japan and China, for which I am indebted to Dr Thin of Shang-hai, to whom they were sent. One of these was obtained from a burial-place of the Aïnos; and as a skull of this race is rarely to be met with, I have thought that it might be of some interest to note its characteristic features, and the points in which it differs from the Mongolian type of skull, on the one hand, and from the ordinary type of Western Europe, on the other; so far at least as the age of the skull, which is that of a child, and its imperfect condition, will permit.

From the condition of the sutures, and the state of development of the teeth, the skull appears to be that of a child about nine years old; the permanent incisors and first true molars being fully developed, while the milk molars are still in their places.

The skull is in an imperfect condition; wanting the nasal, lachrymal, ethmoid, and spongy bones, part of the upper jaws and màlar bones, and the lower jaw. The loss of the nasal, and the mutilation of the malar bones, are the more unfortunate, as on the form of those parts have been founded some of the chief points of distinction between the Aïno and European type of skull.

The denticulations of the cranial sutures are all very simple; the tip of the great wing of the sphenoid bone on the left side has been developed from a separate centre of ossification, as is shewn by the presence of a large Wormian bone articulating with the anterior inferior angle of the parietal. The frontal sinuses are very slightly developed, and all the muscular lines and impressions are feebly marked.

The cranium is ovoid in form and nearly symmetrical, the greatest width being between the parietal eminences; the fore-

[1] Read before the Royal Physical Society, Edinburgh, March 22, 1871.

head is well developed and nearly vertical; the frontal eminences are well marked, and the upper frontal and parietal regions are dome-shaped.

The height and width of the orbital orifice are equal; the bones of the face are broader than in a European skull of the same age; and the zygomatic arches, making allowance for their mutilated condition, seem to point to a somewhat pheno-zygous form of skull; certainly they shew more prominence than in the British skulls, with which it has been compared. The ascending processes of the superior maxillary bones are broad, and the space left between them clearly indicates narrowness and length of the nasal bones.

The length of the palate exceeds its breadth by 0·6 of an inch, a proportion which does not differ materially from that of a European skull at the same age.

Posteriorly, the skull presents a pentagonal form with rounded angles; in this view also the greatest width is between the parietal eminences. The upper part of the occipital region is flattened, and the distance between the occipital protuberance and the foramen magnum is short. The foramen itself is very large relatively to the size of the cranium.

The advanced position of the jugular process, on which Mr Busk founds one of the chief characteristics of the Aïno's skull, does not appear in this specimen, nor indeed does it seem to be a constant character, as is shewn in Dr B. Davis's paper, "On the skeleton and skulls of the Aïnos," published in *Memoirs read before the Anthropological Society,* Vol. III. 1867, 1868, 1869.

Laterally viewed, the principal features of the skull which arrest the attention are its great depth, and the shortness and flatness of the occipital region; the shortness, however, being exaggerated by the depth, and being not greater than in one of the British skulls with which it was compared.

As the only Japanese skull available for comparison with the Aïno's was that of an adult, and one of probably an exceptionally low type, with which, therefore, it would be unfair to institute a comparison, the Aïno skull is here contrasted with two British skulls from the Museum of the College of Surgeons, Edinburgh, one of the same age, the other, perhaps, some

months younger; and also with a Chinese skull of about 7 years of age.

With the assistance of Dr M'Bain, I took a number of measurements of these different skulls, from which the accompanying Tables have been drawn up.

	Aïno.	Chinese.	Br. (1).	Br. (2).
Greatest length	6·4	5·6	6·8	7·0
„ breadth	5·0	5·2	5·2	5·3
„ height	5·3	4·4	5·1	5·1
Horizontal circumference	18·75	17·8	19·7	19·8
Relative capacity[1]	30·3	28·1	31·6	32·1
Index of breadth	78	92	76	70
Index of height	82	78	75	71
	Aïno, æt. 9.	Ch., æt. 7.	Br., æt. 9.	Br., æt. 9.
Zygomatic breadth	4·4	4·2	3·75	3·5

It must be explained, with regard to the Chinese skull, that it presents a considerable degree of distortion, which materially influences the indices of breadth and height, and gives it such an extremely brachycephalic character as a breadth-index of ·92 indicates.

Of these four skulls, the two British are dolicho-cephalic; one of them, however, (No. 1) approaching very closely to the sub-brachycephalic form; the Chinese skull is of an exaggerated brachycephalic type, owing to the cause already mentioned; and the Aïno's is distinctly sub-brachycephalic; but as it has not attained its full development and has almost no frontal sinuses, the breadth is probably somewhat greater in proportion to the length than would be the case in the adult. Still the proportions agree very well with those of the adult Aïno's skull described in Dr B. Davis's paper before mentioned; in these, the breadth-index ranges from ·705 to ·781.

In this point of view then, as may be seen from the table of

[1] The actual capacity of the Aïno cranium is 82 cubic inches; of the Chinese 72 cubic inches.

measurements, the skull of the young Aïno comes nearer to that of the young European than to that of the young Chinese.

In the relative proportion of the three arcs forming the occipito-frontal arch, the Aïno's skull also more nearly approximates to the European than to the Chinese. In the proportions of the palate there is no material difference between the Aïno and the European skull; and as regards the breadth of the skull at its base, the proportions of the Aïno are nearer those of the European than those of the Chinese, in which, indeed, though a younger skull, the breadth between the roots of the zygomatic processes is absolutely greater than in any of the other skulls, with the exception of one of the British (No. 1), which in this respect differs as much from the other British example as the Aïno's does.

The regular ovoid outline of the cranium, the dome-form of the upper frontal and parietal regions, the vertical and well-developed forehead, and the smaller degree of width and flatness of the face, serve further to distinguish the Aïno from the ordinary Mongolian form of skull; in which the outline is more quadrilateral, the forehead lower, and the parietal region roof-shaped and rising to a crest at the sagittal suture, in the adult form at least[1]. In fact, the contrast between the Aïno's skull and that of the adult Japanese is most striking in every respect, and seems to be almost enough, even making all due allowance for the difference in age, to indicate a diversity of race.

But while these differences exist between the Aïno and the ordinary Mongolian form of cranium, the points of distinction between it and the European form are also well marked. In the Aïno, the facial bones are decidedly broader, and the zygomatic arches are more expanded; the ascending processes of the superior maxillary bones are broader and the nasal bones longer and narrower; while the short and flattened occiput and the great proportionate vertical diameter of the cranium, give it an appearance which is not usually met with in European skulls, though doubtless some may be found, as for instance the British skull (No. 1), which approach closely to this form.

[1] Even in the Chinese skull, though certainly two years younger than the Aïno's, the roof-shaped form of the parietal region seems to me to be at least indicated, while in the Aïno's there is no trace of such a form.

Still, in reviewing the general features of the Aïno's skull, it must be considered to differ materially from the common type of Western Europe; and in some respects, as the breadth of the face, the length and narrowness of the nasal bones and the flattened occiput, to be nearly allied to the Mongolian type.

In conclusion, from all that is known, or has been written, in reference to the race of the Aïnos, and their craniological characteristics, it can scarcely be doubted that the skull of the Aïno is of a higher type than the ordinary Mongolian, and in some respects approaches nearer to the European form; but that it is nearly allied to the Mongolian type, if this skull is to be taken as a fair example, appears to be equally certain.

ON THE SO-CALLED TWO-HEADED RIBS IN WHALES AND IN MAN. BY PROFESSOR TURNER.

ALL anatomists, who have given much attention to the osteology of the Cetacea, are aware that in several skeletons of these animals which have been described, more especially in the skeletons of some of the whalebone whales, a peculiar bifurcated subdivision of the vertebral end of the first rib has been observed.

This anatomical peculiarity has been regarded by some systematic zoologists, more especially by Dr J. E. Gray of the British Museum, as a character of so much importance that it has been made a basis for classification. Dr Gray has separated those skeletons of the whalebone whales in which this condition of the first rib has been seen from the species with which they might in other respects have been associated, and erected them, not merely into distinct species, but even into new genera.

This position of Dr Gray's has recently been called in question by Prof. Van Beneden of Louvain, who argues that the bifidity of the first rib in a whale is, like the occasional occurrence of a cervical rib in man, "not a normal arrangement but an accidental conformation, of which zoologists need take no more heed in classification than of anomalies or monstrosities[1]," and that the genera *Sibbaldius* and *Hunterius* ought to be suppressed.

I propose in this communication to bring together and review those facts and opinions which have been collected and advanced by previous writers, and to record some new facts and observations, which may, I hope, prove of service in enabling naturalists to come to a satisfactory conclusion on this subject.

a. In Whales.—In the year 1864 Dr Gray communicated to the Zoological Society of London[2] an important memoir on the classification of the Cetacea, in which he subdivided the family Balænopteridæ into five genera. To one of these he

[1] *Bulletins de l'Academie Roy. des Sciences de Belgique*, II. 1870, p. 820.
[2] *Proceedings*, May 24, 1864.

applied the name *Sibbaldus* or *Sibbaldius*, and gave as one of its most diagnostic characters "the front ribs double-headed." In this memoir he recognised two species in that genus. To one, *Sibbaldius laticeps*, he referred the skeleton of the whale in the Berlin Museum described by Rudolphi, and a skeleton at Leyden: to the other, *S. borealis*, he referred the Ostend whale, and a female stranded in 1840 on the coast of Dorset and described by Mr Yarrell as *Balænoptera Boops*[1]. He has since erected *laticeps* into a distinct genus, to which he has given the name *Rudolphius*[2].

In the same year Prof. Flower communicated to the Zoological Society[3] a description of several skeletons of the cetacea which he had examined in the museums in Holland and Belgium, and on one of these in the Leyden Museum, the skeleton of a whale taken ou the coast of Java, he founded a third species, *Sibbaldius schlegelii*. Three other species have also recently been added by Mr Cope, viz. *S. sulphureus*, *tectirostris*, and *tuberosus*, from whales which frequent the North Atlantic or North Pacific Oceans[4].

As it is of importance for our enquiry that we should possess an exact conception of the conformation of these so-called two-headed ribs, I shall now state concisely the nature of this anatomical peculiarity; and for fuller descriptive details refer the reader to the memoir by Prof. Flower already quoted, and to several short papers by Prof. van Beneden in recent parts of the *Bulletins de l'Academie Royale des Sciences de Belgique*.

In the greater number of the skeletons, referred to the genus *Sibbaldius*, in which this conformation has been observed, the first rib was subdivided at its vertebral end by a deep cleft into two distinct processes, one in front of the other, so that they lay in two different transverse planes. The posterior subdivision articulated only with the transverse process of the first dorsal vertebra, whilst the anterior articulated with the transverse process of the seventh cervical vertebra.

An interesting and instructive variation from this arrangement has been described by both these anatomists in a skeleton

[1] Ibid. Feb. 11, 1840.
[2] *Catalogue of Seals and Whales*, p. 170, and *Supplement*, 1871, p. 54.
[3] *Proceedings*, Nov. 8, 1864.
[4] *Proc. Nat. Hist. Soc. Philad.* 1869, and *Gray's Supplement*, 1871, p. 55.

of *laticeps*, obtained by Eschricht from the North Cape, now in the Museum of Natural History in Brussels[1]. Applied to, and articulating with, the anterior surface of the first right rib was an elongated compressed moveable bone, curved like a rib, whilst the corresponding bone on the left side was united to the left first rib by ankylosis. These bones were without doubt examples of supernumerary cervical ribs; on the one side fused with the 1st thoracic, and on the other a distinct bone; although it is not stated, either by Flower or Van Beneden, that they articulated by the vertebral end with the transverse processes of the seventh cervical vertebra.

From some of the descriptions which have been given of the Ostend whale, and from Rudolphi's own account of the skeleton of *laticeps* in the Berlin Museum, it might at first sight appear as if in these animals the two so-called heads of the first rib had not articulated with the first dorsal and last cervical vertebræ, but with the first and second dorsal vertebræ. Dr Gray, for example, in his summary[2] of the characters of the Ostend whale, founding without doubt on the following passage in Dubar's *Ostéographie*, "mais la première étant biceps, elle s'articule aux deux premières dorsales," states, "the first rib double-headed, articulated to the first and second dorsal vertebræ." But if Dubar's description be carefully perused it will be seen, that he says elsewhere, p. 38, that the two-headed first rib articulates with the transverse processes of the seventh and eighth vertebræ. Now as the cetacea, like the mammalia generally, possess seven cervical vertebræ, it follows that the seventh vertebra should not be regarded, as was done by Dubar, as the first dorsal, but as the last cervical, so that in this, as in the former specimens, the bicipital end of the rib articulated with the seventh cervical and first dorsal vertebræ.

Again, in Rudolphi's[3] description of the specimen of *laticeps* in the Berlin Museum, he speaks of thirteen ribs on each side,

[1] Van Beneden has figured the anterior ribs of this skeleton in the *Bulletins*, 1868, p. 16, Plate I.

[2] *Catalogue of Seals and Wales*, p. 176.

[3] *Abhand. der Akad. der Wissenschaften zu Berlin*, 1822, p. 27. Rudolphi named this whale *Balæna rostrata*, but Dr Gray's specific name, *laticeps*, is more generally adopted.

but of fourteen dorsal vertebræ, so that he makes the vertebra, to which the anterior subdivision of the bicipital first rib is articulated, to belong to the dorsal series. Eschricht, however, has shown[1] that the skeleton of this whale possesses only thirteen dorsal vertebræ; consequently, the vertebra which Rudolphi considered to be the first dorsal is more correctly to be regarded as the last cervical.

Reinhardt also in his essay on Steypireyðr[2], recalls attention to the fact that Eschricht pointed out, more than twenty years ago, an indication of a bifurcation in the upper end of the 1st rib of a whale, which he, and more recently Prof. Flower, have referred to the *Balænoptera musculus;* and during the past year Van Beneden observed[3] a bifid 1st rib in the skeleton of a *Balænoptera musculus* captured in the Scheldt.

But the Balænopteridæ are not the only whalebone whales in which Dr Gray has founded a genus on the double-headed form of the first rib. In the Museum at Leyden is the skeleton of a Southern Right Whale from the Cape of Good Hope, in which Schlegel many years ago pointed out that each first rib was subdivided at its vertebral end by a deep notch into two processes[4]. This skeleton has usually been referred to the genus *Balæna,* or *Eubalæna,* sp. *australis.* In a second memoir on the classification of the whalebone whales[5] Dr Gray erects it into a separate genus, confers on it a new specific name, and terms it *Hunterus,* or *Hunterius, Temminckii.* Since the publication of that memoir Prof. Lilljeborg has given an account of two sub-fossil whales discovered in Sweden[6]. One of these he refers to the genus *Hunterius,* and distinguishes it by the specific name *Svedenborgii,* but he states that both the first and second pairs of ribs were missing from his specimen, so that we have no information on the form of the first rib in this creature. The skeleton of a young whale from the Museum at Pampeluna,

[1] In Lilljeborg's *Essay on the Scandinavian Cetacea,* translated for the Ray Society, p. 269.

[2] *Annals of Natural History,* Vol. II. 1868.

[3] *Bulletins,* 1870, p. 320.

[4] In the articulated skeleton the two heads of this rib are attached to the 1st and 2nd dorsal vertebræ, but Prof. Flower has shown that they ought to have been articulated with the 7th cervical and 1st dorsal.

[5] *Annals of Natural History,* Nov. 1864.

[6] *Nova Acta regiæ Societatis Upsaliensis,* 1868, p. 35.

which Mr Flower states to possess a bifid first rib, has been named by Dr Gray *Hunterius biscayensis*[1].

In the month of October 1870, the Anatomical Museum of the University of Edinburgh acquired, through the liberality of Messrs. J. and J. Cunningham of Leith, a number of cetacean bones, which had been imported into Leith from the Cape of Good Hope. On examination I found them to belong to at least two distinct species—the humpbacked whale of the Cape, *Megaptera Lalandii*, and the Southern Right Whale, *Balæna australis*. One of the bones, a first rib, exhibited in a very characteristic manner the forked condition of the vertebral extremity. It may be useful for purposes of comparison to describe it carefully. It was subdivided at its vertebral end by a cleft twelve inches deep into an anterior and posterior limb. The anterior, curved from the body of the rib upwards and inwards, it was somewhat flattened on its anterior and posterior surfaces, and had its inner and outer borders rounded: the inner border formed a continuous curve with the inner border of the body of the rib. The posterior limb had unfortunately been broken away near the base. The body of the rib was 33 inches long from the bottom of the cleft to the sternal end, which latter had an irregular broken outline, and in all probability was not bifid, as in the rib of *H. Temminckii* figured by Dr Gray[2]. The breadth of the body of the rib, five inches from the bottom of the cleft, was nine inches, and ten inches from the sternal end it was fourteen inches, which was the broadest part of the rib. The outer surface of the rib was somewhat convex. The inner surface, slightly concave, presented a broad vertical ridge which commenced at the bottom of the cleft and extended for ⅔rds of the distance towards the sternal end: on the supposition that this bone was formed by the fusion of two originally distinct ribs, it is probable that this ridge marked their line of union. The circumference of the base of the anterior limb was thirteen inches, that of the posterior about sixteen inches. The chord of the arc from the vertebral to the sternal end was 41 inches.

In the toothed whales, also, the anterior ribs have occasionally been observed to present a similar modification in form;

[1] *Supplement*, 1871, p. 44. [2] Catalogue, p. 99.

but no naturalist has, I believe, attempted to separate these specimens from the species and genera to which they are

Fig. 1.

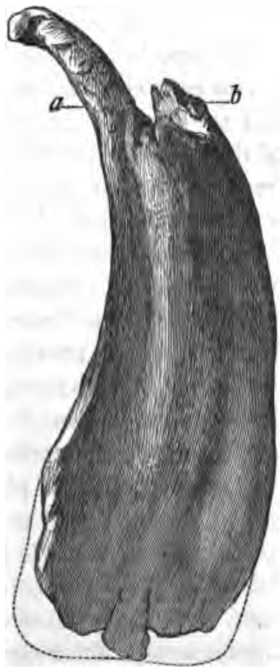


Fig. 1. Pleural surface of the blended ribs in the Cetacean, from the Cape of Good Hope. *a*. The anterior limb, which in all probability had been articulated with the 7th cervical vertebra. *b*. The broken posterior limb of the 1st thoracic rib. The dotted line below marks the probable outline of the sternal end.

usually referred, and erect them into new species. Thus Eschricht found the first left rib in the skeleton of an Orca from Greenland distinctly forked[1]. In more than one species of toothed whale an arrangement has been seen which will aid us materially in determining the true character of the bifurcation. Van Beneden observed in a common dolphin a rudimentary rib on each side connected with the 7th cervical vertebra. In a common porpoise a right cervical rib had an independent articulation by one end with the sternum, and by the other with the

[1] See Reinhardt, in *Annals of Natural History*, Vol. II. 1868, p. 328.

superior transverse process of the 7th cervical vertebra: on the left side the supplementary cervical rib, whilst almost meeting the transverse process by its upper end, did not reach the sternum below, but on the other hand the 1st thoracic rib widened out at its sternal end, and was connected to that bone by two distinct costal cartilages[1]. In these cases a supplementary cervical rib existed on each side, but it had not yet reached the stage of fusion with the first thoracic: had it done so, then the bifurcated form of the first rib would have been produced.

From these observations the following general propositions may be considered as fully established.

1st. In the cetacea cervical ribs are not unfrequently developed in connection with the 7th vertebra.

2nd. The cervical ribs may either remain free, or may become permanently blended with the 1st thoracic rib.

3rd. When a cervical rib is blended with the 1st thoracic rib, the vertebral end is subdivided into two limbs which lie in different transverse planes. The anterior limb either articulates with, or is in the same transverse plane as the 7th cervical, the posterior limb as the 1st dorsal vertebra.

4th. The bifurcated form of the rib is due, not to the subdivision of a single bone into two parts, but to the fusion of two bones into one mass, the vertebral extremity of which continues to exhibit its fundamental duplex character.

b. In Man.—In man, as is well known, ribs are occasionally developed in connection with the 7th cervical vertebra. As a rule these ribs are free, or at least not ankylosed, at their anterior ends, and, except in very rare cases, are not attached by costal cartilage to the sternum. I need not enter here into the characters of these non-ankylosed cervical ribs, but may refer for fuller information to Professor Gruber's exhaustive memoir[2], or to my own Essay in the 4th volume of this *Journal*, p. 130. On this occasion I shall more especially refer to those cases in which two adjacent ribs have been blended together so as to assume the so-called double-headed character.

As far back as the year 1740, M. Hunauld communicated to

[1] Figured by Van Beneden in *Bulletins*, Vol. XXVI. p. 16, 1868.
[2] *Die Halsrippen des Menschen. Mémoires de l'Acad. imp. des Sciences de St. Petersbourg*, 1869.

the Royal Academy of Sciences[1] a memoir on variations in the number of the ribs. He not only figured (Figs. 7 and 8) two specimens of non-ankylosed cervical ribs, showed their mode of production by excessive growth of a free, moveable, anterior transverse process of the 7th cervical vertebra, and pointed out the development of this process from an independent centre of ossification, but he also figured (Fig. 2) a portion of the skeleton of an adult, "in which the 1st rib on each side, well formed posteriorly, and articulated with the 1st dorsal vertebra, became blended with the 2nd rib, which was by this union much larger than usual." The figure, though ill drawn, is yet sufficiently well expressed to enable one to see that in this subject, as in the cetacea, the rib was subdivided at its vertebral end into two limbs, which lay in different transverse planes. The sternal end of the rib was not subdivided.

Sandifort has also figured[2] "costa prima dextra hominis adulti, quæ duo plane distincta capita habet," in which a like arrangement was present. In an important memoir on cervical ribs, Dr Robert Knox not only directed attention to Hunauld's and Sandifort's cases, but described some specimens in his own collection[3]. He is disposed to consider the specimen represented by Hunauld in Fig. 2, and Sandifort's specimen, as analogous to one which he figures (Figs. 2 and 3), and he believes each specimen to be an example of the blending of a cervical rib with the 1st true or thoracic rib; though he admits that, in the absence of any history of the preparation, the explanation, so far, therefore, is open to objections. This supposition, as regards Sandifort's case, has recently been concurred in by Halbertsma, who, from an examination of the specimen, pronounces it to be a cervical blended with the 1st thoracic rib. Knox also represented a specimen (Fig. 5) where the shafts of two adjacent left ribs were intimately blended together so as to form a broad bone. At the vertebral end it was deeply cleft, and presented

[1] Amsterdam, 1744.

[2] *Museum Anatomicum*, Tab. XLIX. Figs. 1 and 2.

[3] These specimens are now in the Anatomical Museum of the University of Edinburgh. At the time when I published the paper on non-ankylosed supernumerary cervical ribs in this *Journal*, Nov. 1869, I described three of these specimens without being aware that Knox had not only described but figured them in the *London Medical Gazette* for Nov. 3 and 10, 1843, Vol. XXXIII. pp. 136, 166. I am indebted to my friend Dr Lonsdale for the reference to Knox's paper. See his *Life of Knox*, p. 336.

two heads, necks, and tubercles; and at the sternal end it was also bifid, and obviously had possessed two distinct costal cartilages. He believed that this also was a case of cervical rib, which, though blended with the 1st true rib, yet possessed a distinct sternal connection; but, in the absence of a knowledge of the requisite details respecting its sternal and vertebral connections, states that "it may be a partial fusion of the 1st and 2nd thoracic ribs."

Huntemüller has recently described[1] a preparation in the Anatomical Museum in Göttingen, which he considers to resemble Sandifort's specimen, and to be a right cervical rib, blended by ossific union with the shaft of the 1st thoracic rib; but distinct as regards the head, neck, tubercle, and hinder part of the body.

But other cases have been put on record which are undoubted examples of the blending together of two or more adjacent thoracic ribs. Hunauld stated that he possessed the skeleton of a fœtus about the 7th month, where the five upper left ribs were united posteriorly, but separated anteriorly; the 6th and 7th were also partially joined together. This observation is important, as it shows that the junction had occurred during intra-uterine life, and in all probability the osseous had been preceded by a cartilaginous union. Sandifort also figured (Tab. XLVII, XLVIII. XLIX) several specimens of ossific union between adjacent ribs, either at their shafts or necks; and in the 4th Volume of this *Journal* Dr Campbell figured (Pl. XI.) several ribs which were united together either by cartilaginous or osseous plates.

In the Anatomical Museum of the University of Edinburgh are two, as yet undescribed specimens, which illustrate the blending of ribs; and, though no history has been preserved in connection with them, yet a description of their anatomical characters may aid in our present enquiry.

In 383 *a*, two left ribs were blended together. The heads, necks, tubercles and $\frac{5}{8}$ths of an inch of the adjacent parts of the shafts were distinct; then the shafts blended and formed a bone $1\frac{5}{8}$ inch in diameter in its broadest part. Anteriorly the bone bifurcated and possessed two distinct costal

[1] *Henle u. Pfeuffer's Zeitschrift*, 1867, p. 149, Vol. XXIX.

Fig. 2.

Fig. 2. Specimen 383 *a* in the Anatomical Museum of the University of Edinburgh.

a. The neck of the upper.
b. The neck of the lower of the two ribs.

cartilages. The length of the shaft of the upper of the two ribs, measured along the convexity from tubercle to costal cartilage, was $3\frac{6}{8}$ inches, that of the lower $6\frac{5}{8}$ inches. The concave border and a part of the upper surface of the broad conjunct body was marked by a deep subclavian groove, in front of which was a well-marked scalene tubercle. The length of the upper sternal limb of bifurcation was $\frac{6}{8}$ths inch, that of the lower two inches. A broad 'intercostal space' separated these two limbs from each other.

Specimen 80 *e* consisted of two blended right ribs and the right halves of the two vertebræ with which they articulated. The heads, necks, tubercles and $\frac{6}{8}$ths of an inch of the posterior parts of the shafts were distinct. Then the shafts united and formed a bone two inches in diameter at its broadest part. This bone had not so distinct a bifurcation anteriorly as the former specimen, for the upper limb almost immediately became continuous with its costal cartilage, whilst the lower was prolonged forward for two inches before it became connected with its cartilage. The length of the shafts of the ribs, measured as in 383 *a*, was, upper $4\frac{3}{8}$ inches, lower $6\frac{5}{8}$ inches. The subclavian groove and scalene tubercle were not so strongly

Fig. 3.

Fig. 3. Specimen 80 *e* in the Anatomical Museum of the University of Edinburgh. *a*. The neck of the upper. *b*. The neck of the lower of the two ribs. The specimen is represented from below, and the drawing much foreshortened.

marked as in the former specimen. In 80 *e*, 383 *b*, and in the specimens figured by Knox (Figures 2, 3, 5), the head, neck and tubercle of the higher of the two ribs closely resembled in form the 1st thoracic rib, whilst the corresponding parts in the lower more nearly approached the form of the 2nd rib, so that at first sight many might be inclined to say that the broad bone formed by their junction was due to the blending of the 1st and 2nd thoracic ribs with each other. Fortunately, however, the preservation of the vertebræ with which the ribs articulated in 80 *e*, enables me to state that this is not the case, and to show that the broad bone is formed by the junction of a cervical with the 1st thoracic rib. The upper head did not, as with the normal 1st thoracic rib, articulate with the upper part of the side of the body of the corresponding vertebra, but with a slightly elevated tubercle at and just above the middle of the side of the body of its vertebra. In this respect it agreed with the arrangement seen in the five specimens of non-ankylosed cervical ribs contained in the University Anatomical Museum. The upper surface of the body of this vertebra had the form of a cervical, the superior articular processes were transversely oval and directed upwards and backwards, and the transverse process, though simulating

in general form that of a dorsal vertebra, and articulating in front with the tubercle of the rib, was directed outwards and a little downwards, and not outwards backwards and upwards, as is the case with the transverse processes of the upper dorsal vertebræ. In its direction this transverse process agreed with those of the 7th cervical vertebra in the specimens, in the museum, just referred to, with which non-ankylosed cervical ribs articulated. The lower of the two heads of the double rib had a double articulation, one half with the lower part of the side of the body of the upper vertebra, the other with the upper part of the side of the body of the lower vertebra, an arrangement of the 1st thoracic rib not unfrequently met with when a cervical rib is developed immediately above it[1].

All these characters leave no doubt in my mind that the upper rib-element is a cervical rib, and that the broad shaft in this specimen, and also that in 383 *a*, are formed by the junction of a largely developed cervical with the 1st thoracic rib. And I believe it may now be considered as proved, what had previously been inferred by Knox, Halbertsma and Huntemüller, but which could only be a matter of inference so long as the vertebral attachments of the two heads were not known, that the specimens described by those anatomists, as well as the one figured by Hunauld (Fig. 2), were formed in a precisely similar manner.

It may be stated therefore for man, as I have already said for the corresponding arrangement in the cetacea, that the bifurcated form of the rib is due, not to the subdivision of a single bone into two parts, but to the fusion of two bones into one mass, the vertebral extremity of which continues to exhibit its fundamental duplex character.

But whilst recognising the general morphological resemblance between these ribs in man and in the cetacea, yet certain features of difference are exhibited, at least by the whalebone whales, to which it is necessary that I should refer. In the *Balænoidea* the vertebral extremities of the thoracic ribs possess no proper 'head,' and as a rule no 'neck,' and are articulated

[1] *e.g.* Cases 1 and 2 in my paper on non-ankylosed cervical ribs, Vol. IV. p. 130. Through a typographical error on p. 133, the rib attached to the left side of the 12th dorsal vertebra is stated to be 8 inches long and 4 wide, instead of 0·8 inch long and 0·4 wide.

to the vertebræ merely by the tuberosity; their ribs, in consequence, correspond only to those parts of the human ribs which we call the tuberosity and the shaft. In man, when a cervical rib is developed, it invariably—whether fusion with the first thoracic rib does or does not take place—consists of head, neck and tubercle, though its shaft may vary much in length; and the rib takes the place of the anterior transverse process of the vertebra. In the *Balænoidea* the cervical rib follows the general plan of development of the ribs in these animals, and possesses neither head nor neck, nor does it take the place of the anterior (inferior) transverse process of the vertebra, and it is articulated, like the proper thoracic ribs, with the transverse process of its corresponding vertebra.

When, in man, a cervical rib is blended with the first thoracic, the single bone formed by their junction is by no means uniform in shape, in size, in the place of junction of the two ribs, or in the mode of connection of its sternal end. In Hunauld's case, in Knox's (Figs. 2 and 3), in Sandifort's, and in Huntemüller's the bone possessed only a single sternal articulation; but in Knox's second case (Fig. 5), and in both the specimens, which I have described in this Essay, the bone bifurcated anteriorly, and each branch possessed its own costal cartilage.

Similarly, in the cetacea, differences of a like kind have been seen, which have not necessarily any specific value, and, although the rib in the specimen from the Cape which I have figured, differs in form from the one figured by Dr Gray as *Hunterius Temminckii*, yet I think it very probable that they are both from the Southern Right Whale.

The consideration of the numerous facts which I have cited and analysed in this paper, has led me to the following conclusions; 1st, that the presence of a cervical rib in man and in the cetacea, whether blended or not with the first thoracic rib, is a mere individual variation; and that just as we should as little think of classifying those men who possess cervical ribs as a genus distinct from the men who do not possess them, so should we as little think to found a genus of whales on the presence of these structures. 2nd. That the genera *Sibbaldius*, *Rudolphius*, and *Hunterius*, so far at least as they are founded on the double-headed character of the ribs, are based on an

inexact conception of the nature of the anatomical peculiarity, and ought no longer to have a place in our systems of classification. My observations therefore support and give strength to the arguments which Van Beneden has advanced in opposition to the view entertained by Dr Gray.

ON THE TRANSVERSE PROCESSES OF THE SEVENTH CERVICAL VERTEBRA IN BALÆNOPTERA SIBBALDII. BY PROFESSOR TURNER.

THE subject of cervical ribs is so intimately associated with that of the form and the development of the transverse processes of the seventh cervical vertebra, that a brief account of some observations which I have recently made on the latter bone, may not inappropriately follow the preceding paper.

In the cetacea the inferior transverse process on each side of the seventh cervical vertebra is wanting; whilst in *B. Sibbaldii*, as in other Fin Whales, the vertebræ in front of the seventh (the atlas being excepted) possess on each side a well-developed superior and an inferior transverse process, which as a rule uniting externally, form with the side of the body the boundaries of a large ring.

In the course of my dissection of the fœtal *Balænoptera Sibbaldii*, obtained from the Longniddry Whale, I had the opportunity of examining the vertebral column in an early stage of its ossification, when the vertebræ were, to a large extent, still in the cartilaginous condition. I found that at this early period the seventh vertebra did not differ in the arrangement of its cartilaginous transverse processes from the vertebræ immediately in front: but that with it, as with them, two cartilaginous bars sprang from each side of the cartilaginous body, and curving outwards became continuous with each other at their outer ends, and formed the boundaries of a large ring.

Hence the differences found, in an adolescent, or adult, Balænoptera, between the transverse processes of the seventh vertebra, and those immediately preceding it, are not due to differences in their original construction, but to a want of ossification of the inferior, transverse, cartilaginous process of the seventh vertebra, so that it either atrophies, or disappears in the process of maceration. Similarly, the absence of a complete ring, which one sometimes sees in connection with the arrangement of the superior and inferior transverse processes of the sixth or preceding cervical vertebræ in the cetacea, is due to an imperfect ossification of their cartilaginous matrix substance. The species and even genera which have been founded on vertebræ which exhibit imperfections in the formation of these rings, are based therefore on specimens in which the process of ossification has not been completed.

My observations on the presence of complete cartilaginous rings in connection with the transverse processes of the seventh vertebra are of especial interest also in reference to a remark recently made by Van Beneden,[1] "that in the dolphins of the tertiary period an inferior transverse process is well developed." Hence it would appear that although the dolphins of the tertiary period differ, as regards the development of the inferior transverse process of the seventh vertebra, from adult cetacea of the present fauna, yet that they correspond with the fœtal stage of some of the now-existing cetacea.

[1] *Bulletins de l'Acad. Roy. des Sciences de Belgique*, 1870, No. 12, 376.

REVIEWS AND NOTICES OF BOOKS.

1. *The Descent of Man and Selection in Relation to Sex.* By C. DARWIN. John Murray, London.
2. *On the Genesis of Species.* By ST GEORGE MIVART, F.R.S. Macmillan and Co.
3. *On Natural Selection.* By A. R. WALLACE. Second edition. Macmillan and Co.

THOSE who have read the philosophical work through which Mr Darwin is best known to the public with the care it deserves, will not be surprised to learn that he had during many years been collecting notes on the descent of man without any intention of publishing, but rather with the determination not to publish anything on the subject, lest what he wrote on that subject should add to the prejudices against his views. A still larger number of people who talk of Mr Darwin's theory as though they had read the *Origin of Species* without having done so, will be surprised to find that the descent of man, which they have all along assumed to be the main feature of his previous work, was the very one which he purposely and studiously avoided. Very superficial as well as very profound minds adopt the maxim, "The proper study of mankind is man," and the undiscriminating public summarizes its ignorance by supposing that Darwin wrote a book to prove that man is descended from the gorilla. By this rough and ready supposition they of course shew that they know nothing either of the matter or manner of the theory of natural selection. The object of Mr Darwin's reticence, which was to disarm prejudice, has therefore been but very partially attained. If he wished to escape the *odium theologicum* by his omission to reason from phenomena found in that species which is "the wonder and glory of the universe," or to apply his reasoning to it, he can scarcely be said to have succeeded. Till the publication of this his latest work his attitude towards those who oppose themselves to his theory, from the mistaken notion that it is incompatible with other truths which they hold with great tenacity and with which they are better acquainted than with the facts and truths with which Mr Darwin deals, has been altogether exemplary. With the exception of the large and designed omission above referred to, his attitude was not indeed conciliatory towards the orthodox, for truth is not conciliatory, but it was never arrogant, presumptuous, or dogmatic; never failed in candour, caution, and moderation. Why both the thinking and unthinking should have jumped so universally to the conclusion that Mr Darwin held precisely the same theory with regard to the derivative origin of man which he enunciated concerning the origin of other species, when he was resolutely silent on the point, is not obvious. It is true that some similar facts to those on which he based his reasoning were to be found in man's well-studied physical frame; but, on the other hand, forcible arguments in favour of natural selection left him untouched, and difficulties which do not

present themselves to the derivative origin of other animals are patent in his case. The very potent arguments derived from geographical distribution, and from the relation of recent geological fauna to the existing inhabitants of the areas where these are found, had no application to man, for man has penetrated everywhere, and no fossil ape has been found which bears a nearer resemblance to him than do the extant apes with which he has been compared. Again, the mental and moral and even the physical constitution of man present some very awkward problems for solution to the advocates of his derivative origin from a lower form. Not only were there grounds for the acceptance of a theory of natural selection so far as animals were concerned while that theory was held to be inadequate to account for the existence of man, but such a doctrine has been actually enunciated by Mr Wallace, by whom the theory of natural selection was first foreshadowed. The public however was right in its assumption, and Mr Darwin has declared his conclusion that man is derived by lineal descent from a lower form not only as clearly and distinctly as we could wish, but as authoritatively and dogmatically as he has propounded the other parts of his theory modestly and temperately. Mr Darwin writes: "The main conclusion arrived at in this work and now held by many naturalists who are competent to form a sound judgment, is that man is descended from some less highly organized form. The ground upon which this conclusion rests *will never be shaken*. . . . It is incredible that all these facts should speak falsely. He who is not content to look, like a savage, at the phenomena of nature as disconnected, cannot any longer believe that man is the work of a separate act of creation."

In his speculation as to the genealogical descent of man and the way in which he emerges from the ancestral tree of the animal creation, Mr Darwin is almost wholly guided by the rudimentary organs found in man. Mr Darwin is quite consistent in this method. No doubt rudimentary organs which are functionless in one species and have dwindled almost to nothing, but are developed and have a palpable use in other allied forms, present the greatest difficulties to those who do not believe in a derivative origin of species, and also afford the strongest support to the selection theory. After enumerating the aborted organs, the transient and fœtal structures, and the often recurring abnormalities found in man, the author works out his theory of origin almost strictly in accordance with the plan of associating the ancestors of man proximately with those species which possess the most of these analogous structures, and so on to those larger divisions in which a fewer number of them have a wider distribution. This plan is, no doubt, philosophical, but it leads the author into some strange speculations. By similar reasoning it is demonstrable that our ancestors were hermaphrodite, and, that long after they had ceased to be so both sexes yielded milk to nourish their young, and perhaps carried them in marsupial sacs. Many of these structures, which on the Darwinian hypothesis must be considered as heirlooms of the species of wondrous antiquity, which man does not cherish but which he cannot lose, and which, like the the slave of the triumphant

emperor, will through long ages check his pride with the reminder, "Thou also art a beast," will be remembered by all. As examples of transient organs, the lunago or hairy covering of the fœtus, extending all over the body except on the palms of the hands and the soles of the feet; the clefts in the neck of the embryo, with the early fish-like disposition of the great vessels proceeding from the heart; the corpora wolffiana and chorda dorsalis may be mentioned. The rudimentary structure which will occur to every one are the cæcum, fragmentary relics of the panniculus of which the plastyrma myoides is the most remarkable, and the nictitating membrane of the eye. The evidence of a tail rests on a double foundation derived from both classes, for the excert termination longer than the limbs in the fœtus may be called a transient structure, while the coccygeal bones which support it after it is included are certainly rudiments. Mr Darwin cites sixteen or seventeen such structures, and there is little that is novel in the citation. The solitary novel feature, which is brought out in a peculiarly Darwinian style, is the point of the ear. This is the point which of all others will be marked by the public. This is the feature which will be seized upon by the popular instinct. It has already run through society like flame among the heather. The ears of ladies, as they sit at the social table, have ceased to "blush at the praise of their own loveliness" and learned to redden as they tell the tale of their own origin. It may be confidently predicted that Darwin's ear will become as notorious as that of Jenkins.

By following out the suggestions which these aborted organs and transient structures embody in a somewhat crude and servile manner, it is easy to see that Mr Darwin could arrive at no other conclusion but that man is a lineal descendant, proximately, of "a hairy quadruped furnished with a tail and pointed ears, probably arboreal in its habits and an inhabitant of the Old World," remotely, "of an animal more like the larvæ of existing ascidians" (living sacs) "than any other known form."

In judging of the portion of the work in which the descent of man is thus traced from the phenomena of his physical frame alone, it must be remembered that Mr Darwin stands, and professes to stand, on a different platform from that which he occupied when discussing the origin of species generally. In his introduction he speaks of his theory as having been adopted by a large number of naturalists, and he treats this theory as though it were accepted and demonstrated. If it were not recognised that Mr Darwin is, in the present work, following a deductive process of which the "Origin" was the converse inductive one, all would certainly pronounce this part of the work very unsatisfactory. The physical phenomena which tell against the theory of man's descent are scarcely touched upon, or dismissed with a few remarks which shew little grasp of their logical bearings upon the discussion. Take as an instance of this the very scanty treatment of the phenomena of arrested development. Arrested development as distinguished from arrested growth might be expected, on *a priori* grounds, to cast a flood of light on the early condition of the species. In the structures which

present arrested development, we have the work of the microscope and of the museum done for the investigator, and done in a more perfect way than he could possibly perform it. In these structures we might expect to find early conditions of an organ enlarged, and, being still included in the living organism, offering themselves to direct experiment not only as to their intimate structure, but also as to their function. Some of these, such, for instance, as flat-nose and epispadias, may corroborate the theory, but very many others, like cleft palate, ectopia vesicæ, imperforate anus, hypospadias, spina bifida, sternal fissures, seem to give quite a contrary testimony. Doubtless these problems, which nature herself propounds, may admit of solutions which leave this theory of the descent of man unimpaired, and "pangenesis" may be such a solution; but surely the matter not only admits of but requires discussion. It is by no means clear why cleft palate is so often asymmetrical. According to Mr Darwin's theory every transient condition of an organ is not only a means to an end, but once was an end in itself; but, on the other hand, in many cases of arrest of development, we have the end known and the means patent, but the latter so presented as to shew that it never could have been anything else but a means to accomplish the very end of which it failed To dismiss the whole of this subject with the remark that microcephalous idiots are prognathous and fond of running on all-fours, seems unworthy of the author of the *Origin of Species.* Mr Darwin's defence no doubt is that his more recent works are the amplification of his grand sketch—the application of a demonstration established elsewhere—the synthesis of his former analysis;—nevertheless, after all the admissions in Mr Darwin's favour with regard to the matter of his present treatise, there is a falling off from the superlative excellence of his other works, as to the manner of it, in the direction of the faults of crudity and dogmatism, which discordant faults are so often combined in the productions of less careful authors.

Mr Darwin's doctrine, of course, involves metaphysical and moral problems hard to solve and demonstrate, but the author seems to consider the difficulty arises from the solutions and demonstrations being hard to find, and not because the doctrine which involves them is, in the least degree, doubtful. Mr Darwin admits that the difference between the highest apes and the lowest savage is immense, and this mental severance suggests some error in the conclusion to which the study of his bodily structure has led him; but, in shewing that memory, imagination and reason are possessed by brutes, Mr Darwin does not address himself with sufficient attention to the most difficult step in the problem of mental evolution.

Rightly or wrongly the power of forming, and reasoning upon, abstract ideas has been thought to be a faculty differing not only in degree but also in kind from any exhibited by brutes; and here is the gulf Mr Darwin ought to have bridged. Doubtless, much might be written to shew that the power of abstraction is intimately associated with the use of language, and is dependent rather upon the rapidity and precision of ordinary processes of thought than an

evidence of a different power, but Mr Darwin has contributed nothing, or next to nothing, to this demonstration. The matter which appears under the head of "abstraction," and which is almost exhausted by the comparison of an old hound reflecting on the pleasures of the chase, and the Australian wife who uses hardly any abstract words, and cannot count above four, is quite inadequate, and rests upon a positive assumption with regard to the brute and a negative one with regard to the woman which are quite unproved.

That other great problem of the evolution of the moral sense is treated with far greater ability, and one of the most interesting chapters of the work is devoted to an explanation of the production of the human conscience. The existence of the moral sense in man is traced to those social instincts which man has in common with all gregarious animals. The strengthening and growth of the memory and judgment would enable man to compare his past actions, and the more abiding satisfaction of acts prompted by some motives as compared with those prompted by others would create a distinction between the higher and the lower law, or motive, which is all that some modern moralists require. "Ultimately a highly complex sentiment having its first origin in the social instincts, largely guided by the approbation of our fellow-men, ruled by reason, self-interest, and in the later times by deep religious feeling, confirmed by instruction and habit all combined, constitute our moral sense or conscience."

It is unfortunate that the subject of the first portion of the work is so very fascinating to the public that it quite overshadows the far more valuable portion which treats of sexual selection. In dealing with this subject Mr Darwin is himself again. Here caution again tempers his courage, and a manifest candour in stating the whole case gives weight to his conclusions. Again, we have, as in the *Domestication of Animals*, a repertory of facts, carefully collected from the whole range of the animal kingdom. The dogmatic is once more exchanged for the inductive style.

The writer endeavoured to point out the difficulties which the phenomena of beauty presented to the acceptance of the theory of natural selection even when supplemented by the theory of sexual selection, in a review of Mr Wallace's book which appeared in the last No. of this *Journal*, and also in some critical notices of that and the present work of Mr Darwin which appeared in the *British Quarterly Review*, from which a few of the sentences which follow are transcribed.

"Beauty as distinguished from use has always been a stumbling-block to the disciples of the natural selection school. That which, in any species, pleases our minds by the immediate agency of the senses, as distinguished from that which is of service to that species in adapting it to external conditions, is quite unaccounted for by the survival of the fittest, at least so far as wild and untamed species are concerned. Some evolutionists would cut the knot by denying the evidence of beauty apart from fitness. Suitability, symmetry, conspicuousness, and an imposing appearance, are, no doubt, desiderata which

natural selection may seize upon and secure, and these may incidentally and necessarily involve that which is beautiful in our eyes. But after all these have been eliminated or satisfied, there yet remains in a large number of species an element of beauty the contemplation of which brings pleasure to all human beings, whether educated or uneducated, refined or unrefined. This is especially the case throughout those large, numerously represented, and dominant classes taken from two separate sub-kingdoms, and called insects and birds. These two classes occupy a great deal of the attention of Mr Darwin. If we assume any evolutionary theory, and abjure the doctrine of final causes, all the varied beauty of butterflies and humming-birds have but one probable explanation, namely, that of sexual selection. To make even this explanation possible, we must assume a keen, discriminating æsthetic faculty in animals which is like in quality with our own, as that faculty is possessed by the most refined of our species. Moreover, this faculty must be intimately connected with the sexual appetency in each species. Such a connection is, judging from analogy, not improbable. In forming an opinion how far these views are correct, it is important to isolate the operation of sexual selection from that of natural selection. Nature has throughout almost the whole animal kingdom afforded to us the means of isolation. For, as a general rule, the sexes in species are not absolutely alike, and often there is great difference between them. All sexual peculiarities therefore which cannot be explained on the principle of division of labour, throw light upon the æsthetic faculty of animals as a selective, and therefore by the theory, of a creative agency. Mr Darwin has collected a vast mass of facts about sexual peculiarities, which being in no way connected with the sexual function, he calls *secondary sexual characters*. Of course, sexual secondary characters so limited point to a difference in the modification of the sexual desire by æsthetic appetite in the two sexes. Generally speaking, the adorned sex is the male. Have, then, the females a greater appreciation of beauty than their males? Mr Darwin thinks the ardour of the male destroys his discrimination. Some facts produced, however, seem to run directly counter to this supposition. On all hands the peacock is considered the most splendid of birds, and the difference between the sexes in this species is carried to an extreme point. Yet, one of Mr Darwin's best authenticated facts is, that the pea-hen differs from most birds in being the ardent wooer.

"One of the happiest and most satisfactory episodes in the book is the account of the genesis of the eye-spot in the plumage of birds, and specially of that of the ball-and-socket ornament in the secondary wing-feathers of the Argus pheasant. The treatment of this subject reminds us, by its clearness and beauty, of the author's treatises on coral islands and the fertilization of orchids. How simple a phenomenon may disclose a world of interest and wonder when in the hands of a man of genius! It seems to us, however, that that wonderfully faithful representation of a round ball lying in a hollow socket, expressed on the flat of the web of a feather, offers a striking example of the inadequacy of either natural or sexual selection to

explain such phenomena. 'That these ornaments,' says Mr Darwin, 'should have been formed through the selection of many successive generations, not one of which was originally intended to produce the ball-and-socket effect, seems as incredible as that one of Raphael's Madonnas should have been formed by the selection of chance daubs of paint made by a long succession of artists, not one of whom intended to draw the human figure.' Exactly so! We must attribute to the hen Argus pheasant the æsthetic powers of a Raphael in order to account for the decorations of her mate, or, more properly, we must assign to a succession of multitudes of generations of birds a correctness of appreciation of the draughtsman's art, such as is a rare excellence among men. This may be a fact, but if so, it opens up a new realm to our investigation."

Some very novel conclusions are incidentally arrived at in the course of the main discussion, and yet if we do not reject the hypothesis of sexual selection altogether they appear quite legitimate. It would hardly have been supposed that the stridulating organs of the male cicada, whose loud ingratitude for the boon of a silent wife disturbs the stillness of the Italian groves, were intended not to call the attention but to charm his mate. Beautiful as they are in their sweep and contour one would yet hardly have supposed that the horns of deer and antelopes had an ornamental quite as much as a defensive function, or that, generally speaking, the arms of the males are employed so little in international and so much in civil warfare, if these adjectives may be used for interspecific and internecine. That the differences of the sexes should so little depend on what was complementary to the partnership and so much to the exegencies of a competition which is of no service to the species, is a curious fact; and that these differences should be due to some quite unexplained cause whose action admits of such an unlimited diversity both of degree and quality, is more curious still. To explain this last sentence it may be necessary to state that this tendency to secondary sexual differences, transmitted to one sex and not to the other, is, according to Mr Darwin, an ultimate law without which neither natural nor sexual selection could conserve these differences, and yet a law which has a different action in each species, graduating from a persistent zero to cases in which it is enormous. This enormous sexual difference is, further, not a like enormity. It may be in size, as in the case of some seals, or in colours, as in some butterflies, or in vocal power, as in birds, or in a thousand other ways. This view of the question gives rise to a curious and somewhat subtle difference of opinion between the two great advocates of natural and sexual selection. Mr Wallace thinks that in the case of splendid cock-birds who have plain hens, who sit on open nests, the tendency for both sexes to become brilliant has been checked by natural selection. On the other hand, Mr Darwin thinks that secondary sexual splendour was from the first developed only in the male and transmitted by him to his male offspring alone; and in the converse case, where the female is also gay, natural selection causes her to build a covered nest for protection. Mr Darwin grounds his views on the difficulty, if not the impossibility, of any kind

of selection establishing and developing a peculiarity and retaining it in one sex only, if this peculiarity persistently reappeared in the offspring of both sexes, as it most certainly would do if there were no other law to modify the law that like begets like with only minute and fortuitous differences. His view also derives support from the greater likeness of the females to the young in the same species, and their greater likeness to one another throughout many allied species, and in the case of birds with a double moult, the retention of the character of their plumage by the females and not by the males. These considerations seem logically conclusive from the premises, and it is a matter of disappointment that Mr Wallace in the recent Edition of his work has not noticed them. Mr Wallace has, however, introduced into the new Edition of his work a singular speculation with regard to birds, which gives some support to the idea of sexual division of labour, which is the rival of sexual selection in the explanation of secondary sexual differences. This speculation is taken from Mr Richard Spruce, who thinks old birds pair with young ones just as some Indians do, in order that there may be some experience in the family.

It is somewhat unfortunate that the elaborate criticism of Mr Darwin's hypothesis by Mr St George Mivart was published before these last volumes on man's descent. Not that Mr Mivart would find much to retract after the perusal of them, but he would probably find much more to insert. His volume is certainly as good a compendium of the difficulties of the Darwinian theory as has yet appeared, unless we except the article in the *North British Review* for June 1867 and Mr Darwin's works themselves. Mr Mivart seems to have had three objects in view in writing the book: 1. To criticise the Darwinian hypothesis. 2. To establish an evolutionary hypothesis of his own. 3. To reconcile this hypothesis with strictly orthodox views of religion. The first object, however, is that which he has best succeeded in attaining. In the second he appears to have failed, and the third is not much advanced by his method of treatment. The endeavour to show that evolutionism was a cherished idea of the early Christian Fathers, and the cutting of the knot by the dogma that whether the bodily form of man were derived from a lower one by any special Providence or not, "the soul of every individual man is absolutely *created* in the strict and primary sense of the word," without defining what the soul is and what its powers are, will hardly satisfy either scientific men or theologians. Mr Mivart's special theory of evolution may be thus summarized. Evolution proceeds from some internal force directed towards definite ends, and its process is by sudden and distinct and not gradual changes. He adopts for his theory Mr Galton's simile of a spheroid whose spheroidity is due to the multitude of planes which bound it, which resting on a plane is in stable equilibrium and remains so till some force causes it to revolve on to an adjacent plane to that on which it previously stood; while Mr Darwin's theory must be represented by a perfect sphere in neutral equilibrium. Prof. Humphry in his address on Physiology at Nottingham (Vol. I. of this *Journal*, p. 12)

had already suggested this staircase progression of the transmutation of species by an analogy taken from the inorganic world, wherein the successive augmentation of the negative element in the various oxides of nitrogen is made to apply to the succession of species. Mr Mivart however has produced scarcely any facts to support this theory, and has dealt very feebly with the facts adduced. The instance of the sudden appearance of the black-shouldered peacock (Pavo nigri pennis) is the almost solitary instance quoted of an order of phenomena which would attract general attention. Inasmuch too, as the fixity of species as defined by the sterility of hybrids, is one of the objections advanced by Mr Mivart against the Darwinian hypothesis, he was bound to show that the black-shouldered peacock was infertile with the ordinary kind, yet he has not done this. When we compare the few and scattered evidences in favour of abrupt transmutation, which have been far more ably summarized by Mr Darwin himself than the present author, with the almost unlimited evidence of the accumulation of minute and fortuitous variations to an almost unlimited extent, Mr Mivart's theory sinks into insignificance beside the rival theory he criticises. It is not meant that the Darwinian theory is necessarily the correct one in contradistinction to what may be called evolution by cascade, but the labours of Mr Darwin in collecting facts in support of his theory have been enormous, and those of Mr Mivart in support of his theory almost nil.

The specialty of the work then is its criticism of Darwinianism. This criticism can however scarcely be called an assault either in manner or force. It is rather a siege in which the whole *enceinte* is invested, but no practical breach has been made, or at least none which has been followed by an overwhelming onslaught. Like the Robin Hood of *Ivanhoe* he has tried with his shafts every joint in the Norman armour of his Front de Bœuf, and with nearly a like result. His objections, though separately well put, have for the most part been urged before, and some of them seem to be ill-considered and self-conflicting. Thus the hood of the cobra and the rattle of the rattlesnake are given as structures which natural selection would not only not evolve but would suppress as injurious to their possessors. These very objections have been stated by Mr Darwin himself and many after him, and a little thought might furnish a solution. When we consider the habit of reptiles in general, and of snakes in particular, and remember that they are capable of rapid motion for a short time and within a short compass only, and are slow and inert in the intermediate time, we think it by no means certain that the sound produced, which is more like a hiss than a rattle, may not be a valuable aid to a predacious serpent. Creeping along in search of small animals which are hidden while at rest by similative colouring but visible at once when any motion is made, it every now and then emits a sound which at a distance is not very noticeable, but when near is very startling, and so attains its object; for the start of the quarry when within range of the rapid darting motion of its neck would reveal it without enabling it to escape. The hood of the cobra again, dilated at the moment of its striking, is

probably not only a means of appalling its foe, but also of absolutely preventing that foe from seizing the neck, which is just the seizure which would paralyze all the actions of the serpent.

Mr Mivart inquires why the camelopard is the only ungulate animal in South Africa which has an elongated neck. The same conditions ought to have produced the same result in other species. On the other hand he points out very well and truthfully the similarity of structure of the production of the larynx across the pharynx in the young kangaroo and the dolphin. Now these objections seem to neutralize one another, and both of them rather strengthen the hypothesis of natural selection; for adaptive characters are likely to appear in every species, but when they bring their owners into competition they will be likely to be suppressed in all save in that species where the most perfect adaptation is present, while when they do not do so they will all be retained. Long-necked ruminants would be forestalled by the giraffe in South Africa, but the dolphin and kangaroo are not competitors at all, and if they were they would not compete in this particular.

Mr Mivart has also produced some numerical calculations to show the improbability of concurrent variations and the probability of numbers overbearing slightly useful variations. In the first calculation we meet with the following equation, which will astound our mathematicians: $\frac{1}{m^e} = \frac{1}{1000^{10}} = \frac{1}{10^{30}}$. Mr A. Bennett, in his controversy with Mr Wallace, anticipates Mr Mivart; but in such calculations the statement of the case is everything, and in these instances the statements can be shewn to be inexact.

Notwithstanding these and similar shortcomings, *The Genesis of Species* is a very interesting book, and will be a standard contribution to this alluring study. Further inquiry into the origin of species is pressing the scientific world towards the conclusions that while the facts of nature give evidence of evolution, and natural selection is a *vera causa* in that evolution, it is but one cause among many others, which causes for convenience sake may be called laws, but which are but the indications of the orderly procedure of a divine agent. All the facts are not only consistent with, but what might have been expected of the theory, otherwise suggested, of a Creator who sees the end while we only see the process; who knows the means where we only see the end to be desirable; and who has left on that nature which is at once means and end the indelible mark of His own operation.

NEVILLE GOODMAN, M.A., St Peter's College, Cambridge.

Die Lumbal-gegend in Anatomisch-Chirurgischer Hinsicht. Von Prof. LESSHAFT. Berlin, 1870.

IN this memoir, reprinted from *Reichert u. du Bois Reymond's Archiv*, Professor Lesshaft gives an excellent anatomical description of the connections of the muscles and fasciæ in the lumbar region. He

relates a number of observations made to ascertain the position of the colon, in connection with the performance of the operation of colotomy, and he appends a table of 92 cases in which that operation had been performed.

Handbuch der Lehre von den Geweben des Menschen und der Thiere. Herausgegeben von S. STRICKER. IV. Lieferung. Leipzig, 1870.

THE fourth part of this excellent manual of Microscopic Anatomy contains the following articles: The Mammæ, by C. Langer. The external male and female Organs of Generation, by E. Klein. The Spinal Marrow, by J. Gerlach. The Brain of Mammals, by Th. Meynert. The Sympathetic Nervous System, by S. Meyer. The Organ of Taste, by T. W. Engelmann. The conclusion of an article on the Serous Membranes, by E. Klein, and the commencement of one on the Organ of Hearing, by J. Kessel. Meynert's account of the minute Anatomy of the Brain is of especial interest, and we hope soon to have to welcome a translation of this important monograph, from the pen of Mr Power, in the volume about to be published by the Sydenham Society.

L'ordre des Primates, parallèle anatomique de l'Homme et des Singes. Par M. Paul Broca, Paris, 1870.—*Beiträge zur Anatomie des Hylobates leuciscus, und zu einer vergleichenden anatomie der Muskeln der Affen und des Menschen.* Von TH. L. W. BISCHOFF. Munich, 1870.

IN these two memoirs the relations of Man to the Anthropoid Apes, and the position which ought to be assigned to him in the classification of the Mammalia, have been discussed by two eminent anatomists, but with very different results, as regards the conclusions to which they have arrived. M. Broca agrees with those zoologists who regard man as a member of the order of *primates*, in which he forms a distinct family. Like Prof. Huxley, he considers that man differs less from the so-called quadrumana in general, and the anthropoids in particular, than the apes do, one from the other. He compares man anatomically with the apes, and examines the osseous, muscular, vascular, visceral and nervous systems. In his chapter on the nervous system, he figures the brain of a male chimpanzee, in which in the left hemisphere the superior bridging convolution was very distinct, whilst in the right the second bridging convolution was also superficial. In these points this brain closely corresponded with one, figured some years ago by the writer (*Proc. Roy. Soc. Edin.* 19 Feb. 1866). Though he believes that but slight structural differences exist between the corresponding organs of man and the anthropoids, yet he admits that, in the functions which they perform, man is raised to a height far beyond that of the family of anthropoids which most closely approaches him.

Professor Bischoff, though mainly devoting his memoir to the consideration of *Hylobates leuciscus*, more especially its muscular and cerebral anatomy, yet discusses also the general question of the comparison between man and apes. He repeats here, what he believes he has proved in a previous memoir on the convolutions of the brain (*Anatomical Report*, IV. 157), that an uninterrupted series may be traced from the lemur to the orang in the improvement of the convolutions, but that between the brain of man and the orang there is a chasm, which cannot be filled up. His study of the muscular system has now led him to the conclusion that, in the arrangement of the muscles of man and of the anthropoids, there are greater differences than are found in the muscles of the different families of the apes, and, as the anatomical structure of a muscle ought not to be considered apart from its physiological action, the apes stand much nearer to each other than the highest ape does to man. He follows out therefore, to some extent, the line of argument which was pursued by the late Professor Goodsir in his Lectures "on the Dignity of the Human Body[1]," who whilst fully recognizing the importance of the study of morphology, yet vigorously contends that in all questions of general zoology and anthropology the teleological aspects of the question ought not to be overlooked.

[1] *Anatomical Memoirs*, Vol. I. p. 207, Edinburgh, 1868.

REPORT ON THE PROGRESS OF ANATOMY.

BY PROFESSOR TURNER[1].

OSTEOLOGY.—Wenzel Gruber makes in *Bulletin de l'Acad. imp. des Sc. de St Petersbourg*, Oct., Nov., Dec. 1870, several contributions to the OSTEOLOGY OF THE HAND AND FOOT. In the right hand of a man 11 carpal bones were found; the bones of the first row, the trapezium, the first and fifth metacarpals and all the phalanges, were normal: the trapezoid somewhat deformed; the os magnum subdivided into three secondary bones, a superior, a radial and an ulnar: the unciform possessed a radial surface somewhat different from normal: a small bone was situated on the dorsum of the ulnar border of the second metacarpal: a minute nodule of bone was situated in the interosseous ligament which connected the ulnar secondary part of the os magnum with the superior secondary bone and the uncinate: the brachial surface was abnormal in the third and fourth metacarpals. The carpal bones lay in three rows: the first normal: the second formed of trapezium, trapezoid, superior part of os magnum, unciform: the third of epiphysis of second metacarpal, and radial and ulnar subdivisions of os magnum. He describes also a second case of os intermedium, or centrale, analogous to a bone seen in many quadrupeds, in the hand of a man. Also cases in which a persistent styloid epiphysis of the third metacarpal formed a ninth carpal bone, and a case in which this process was partially anchylosed with the third metacarpal. Also a case in which the carpal scaphoid, at one time subdivided into two secondary bones had become, by their union, a single bone. A similar case in which two secondary semilunar bones had united into a single bone. A case in which a persistent styloid epiphysis of the third metacarpal had become anchylosed with the os magnum. A case in which the right tarsal scaphoid from a boy, aet. 13, had its tuberosity in the form of an epiphysis, connected with it by cartilage. Other cases of secondary scaphoid and semilunar bones are described by W. Gruber in *Reichert u. Du Bois Reymond's Archiv*, 1870, p. 490: on p. 494 a cuneiform, on the inferior surface of which a furrow seemed to indicate its original subdivision into two secondary portions: and on p. 499, a case in which a sesamoid bone was developed between the dorsal surface of the ulnar angle of the unciform and the fifth metacarpal, adjoining the insertion of the tendon of the extensor carpi ulnaris. ——Messrs. Pye-Smith, Howse and Davies-Colley (*Guy's Hospital Reports*, 1870) record a case where an *extra sesamoid bone* was developed in the *glenoid ligament* of the *index* at the junction of the first phalanx with the metacarpal bone: a case of *bifurcation of the third left costal cartilage: a case of extensive ossification of the stylohyoid*

[1] To assist in making this Report more complete, Professor Turner will be glad to receive separate copies of original memoirs and other contributions to Anatomy.

ligament: a case where the *vertebral groove* on the posterior arch of the *atlas* was converted into a *foramen:* and, what is most important, a case of ADDITIONAL DORSI-LUMBAR VERTEBRA. This bone had the characters of a lumbar-vertebra, with a bone attached to its left side by a synovial joint three-fourths as long as the last rib, flat from before backwards and tapering, but not curved: no superior transverse process, but a pair of inferior, in a line with those of the succeeding five normal lumbar vertebræ. Then came four anchylosed and one separate sacral vertebra and, lastly, the coccyx. An additional pair of small nerves which joined the sacral plexus was placed between the last moveable vertebra and the sacrum.——In a paper on ARTIFICIAL DEFORMITY of the FEET of CHINESE WOMEN (*Archiv. fur Anthropologie,* 1870,) H. Welcker describes and figures the changes which occur in their osteological structure.——In *P. R. S.*, London, March 16, 1871, G. W. Callender gives in abstract his observations on the formation of some of the SUB-AXIAL ARCHES IN MAN. He describes those which grow into the cervical region in that period of their growth which lies between the fifth and twelfth weeks of fœtal life; he terms these the fourth or lingual, the fifth or hyoid, the sixth or laryngeal, and the seventh or exoccipital. The last he states grows from the basi and exoccipital cartilage regions and ends in two processes, the clavicle and scapula.

CONNECTIVE SUBSTANCES.—W. Flemming describes in *Schultze's Archiv,* VII. 32, the DEVELOPMENT of the FAT-CELLS in the connective tissue, and their mode of degeneration. Observations on the structure of the connective tissue itself are also given.

MYOLOGY.—Messrs. Pye-Smith, Howse and Davies Colley record (*Guy's Hospital Reports,* 1876) the following MUSCULAR VARIATIONS. A subdivision of the *trachelo-mastoid* into a part superficial to and a part deeper than the occipital artery: a subdivision of the *levator scapulæ,* which blended with the serratus magnus: a flat muscle *between the external and internal oblique,* extending from the cartilage of 12th rib to iliac crest: an *additional stylo-hyoid muscle:* a *petro-pharyngeal-muscle* from petrous bone to wall of pharynx, where it blended with middle constrictor: an *additional posterior belly to the sterno-thyroid.* A case in which the *thyro-hyoid was absent* and sterno-thyroid was prolonged to hyoid bone: a muscular slip from the pectoralis major to the capsule of the shoulder-joint, the *tensor articuli humeri* of Gruber: three cases of a *sternalis* muscle, all unsymmetrical: two of a *supra-costalis anterior* (*rectus-thoracis*): a fasciculus from third rib to scalenus medius: a *coraco-clavicularis* from the apex of the coracoid process to the under surface of the clavicle, it stood in the place of the subclavius: and a number of variations in the muscles of the upper and lower limbs.——Wenzel Gruber communicates (*Mém. de l'Acad. Imp. des Sc. de St Petersb.,* 1870) an account of the MUSCULUS ANCONEUS QUINTUS in man. He employs, as is sometimes done by the German anatomists, the terms *anconeus primus, secundus,* and *tertius,* to express the three well-known heads of the triceps extensor cubiti; he speaks of the accessory portion,

which arises from the epi-condyle and above that from the humerus as anconeus quartus; he then describes a very unusual fasciculus, which he has only seen in two cases, as an anconeus quintus. This fasciculus arose in both instances from the anterior surface of the tendon of the latissimus dorsi, and in one, in addition, from the infra-glenoid tubercle of the scapula: it passed down the arm to end, in one case, in the muscular part of the long head, in the other, at the inner border of the long head at its junction with the inner head of the triceps. He considers the quintus to correspond with a subdivision of the extensor cubiti found in many quadrupeds. He gives the name *anconeus sextus* to the muscle formerly described by him as *epitrochleo-anconeus* (*Report*, II. p. 166).

BLOOD-VASCULAR SYSTEM.—A memoir on the DEVELOPMENT of the SEPTUM AURICULARUM CORDIS, by Julius Arnold, is in *Virchow's Archiv*, LI. 220. This septum is composed of a *pars carnosa* and a *pars membranacea*. The former arises at the beginning of the third month as a muscular crescentic fold from the anterior wall of the auricle; the upper, shorter horn, *crus carnosum sup.*, extends backwards along the upper wall of the auricle; the lower longer horn, *crus carnosum inf.*, along the base of the fully developed ventricular septum. These structures increase in size so that at the end of the sixth month the anterior halves of the two ventricles are completely separated from each other, whilst posteriorly they freely communicate. The pars membranacea arises in the form of a crescentic fold in the left wall of the inferior cava. It lies originally to the left of the pars carnosa and lies obliquely in the left auricle. It is at first very shallow, but grows deeper and sends a superior crus along the upper wall, an inferior within the area of the left auricle. The pars membranacea grows forwards towards the pars carnosa, and their respective crura cross each other above and below; then the free borders of the central portions of each part approach and more or less cross, so that the free edge of the pars membranacea lies forward in the left auricle, that of the pars carnosa backwards in the right. A lateral approximation of the membranous to the fleshy part then occurs, and at the same time the former becomes more perpendicular. Along with this change in the position of the pars membranacea septi the place of opening of the inferior cava changes, for whilst originally it opens into the left auricle, later on it ends entirely in the right. The paper concludes with an elaborate analysis of the various modifications in the form and arrangement of the auricular septum which have been seen in man.——Messrs. Pye-Smith, Howse and Davies-Colley record in *Guy's Hospital Reports*, 1870, the following VARIATIONS IN THE ARTERIES: two cases, where the four great arteries arose directly from the transverse part of the arch, the *right* subclavian being the last branch, proceeding from the descending part of the arch and passing between the œsophagus and spine to reach its usual position: a case where the absence of a facial artery was replaced by a large anterior branch of internal maxillary, which passed forward from under the masseter, and by the infra-orbital

artery: various modifications in the arrangement of the arteries of the upper and lower limbs, in one of which a large obturator supplied the place of the left internal circumflex artery.——Wenzel Gruber describes (*Reichert u. du Bois Reymond's Archiv*, 1870, 484) a case in which the DORSAL part of the left RADIAL ARTERY extended as a subcutaneous vessel from the lower end of the forearm to the space between the first and second metacarpal bones.

NERVOUS SYSTEM.—G. H. Meyer records two forms of VARIATIONS IN THE ARRANGEMENT OF NERVES (*Reichert u. du Bois Reymond's Archiv*, 1870, 395). In one the long saphenous nerve ended immediately below the knee in branches to the skin, whilst a branch from the tibial nerve supplied the skin lower down the inner side of the limb. In the other the obturator nerve had two roots.——In the same *Archiv*, p. 501, W. Gruber relates his enquiries into the branch or branches of COMMUNICATION BETWEEN THE MEDIAN AND ULNAR NERVES in the fore-arm of man and mammals. The connection was found at the upper part situated between the superficial and deep flexors of the fingers.——Messrs Pye-Smith, Howse and Davies-Colley report on NERVE VARIATIONS seen in the Guy's Hospital dissecting room (*Reports*, 1870). The most important are the following: The lachrymal instead of the nasal supplied the long root to the ophthalmic ganglion; a branch from descendens noni to sternal head of sterno-mastoid; nerve to serratus magnus formed by branches from fifth, sixth, and seventh cervical nerves; a branch between the ulnar nerve and anterior interosseous of median.

P. Rudanowski contributes to *Virchow's Archiv*, LII. 193, a paper on the STRUCTURE OF THE AXIAL CYLINDER IN THE SPINAL NERVES. He considers that he has proved the presence of special tubes in the form of the axial cylinder in the interior of the primitive nerve-tubes.——Boldyrew of Kasan relates (*Schultze's Archiv*, VII. 166) some observations on the MODE OF TERMINATION OF THE NERVES IN THE LARYNGEAL MUCOUS MEMBRANE, also on the distribution of the blood and lymph-vessels.

EYE-BALL.—Fr. Merkel communicates to *Reichert u. du Bois Reymond's Archiv*, 1870, 642, a paper on the BACILLARY LAYER OF THE RETINA. He considers it to be proved by the observations of various observers that, in the different classes of vertebrata, the rods and cones are subdivided into an outer and an inner segment, that the outer segments have transverse markings which indicate a subdivision into small round discs, which form a reflecting apparatus serving for the concentration of the light. In the substance of the inner segments, in many animals, lens-like bodies also occur, and in the elements of the outer granular layer is to be found a reflecting apparatus in the form of biconcave and biconvex lenses. His own observations are especially directed to the determination of the presence or absence of an investing membrane for the outer segments of the large rods of the amphibia, and the nature of the delicate threads in the retina considered by Schultze to be nerve fibrillæ. He had no

difficulty in seeing the longitudinal markings on the rods, also their subdivision into thin discs, but was unable to demonstrate with certainty the presence of a membrane on the outer segments, though the inner segments were undoubtedly invested by a membrane. He agrees with Schultze's description of the cones in the frog and triton (*Reports*, II. 168); but he does not coincide with him in regarding the delicate threads, either as the endings of nerves, or as continuous with the outer segments of the rods, but that they pass up between the adjacent segments, and through their presence occasion the longitudinal markings or *Cannelirung*, as he terms it, which those rods exhibit.——E. Landolt records (*Schultze's Archiv.* VII. 81) his observations on the RETINA OF THE FROG, SALAMANDER, AND TRITON. He agrees with those observers who have recognised the presence of an investing envelope for the rods and cones. In the outer segments of the rods he has seen a longitudinal striping similar to that described by Max Schultze, and even more frequently the transverse markings in these segments. The lens-like bodies he has also found in all the rods and cones of these animals. Like Merkel he considers the supporting tissue (*stützgewebe*) to be of the nature of connective tissue, and not nerve fibrillæ.

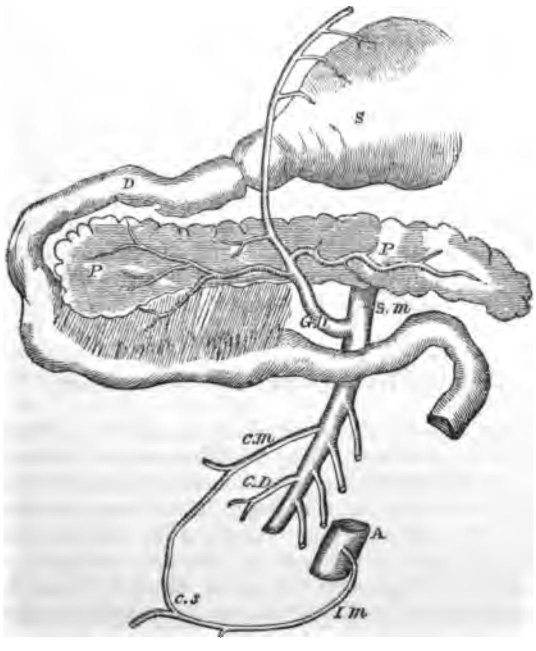
GLANDS.—W. Ebstein records observations on the structure and function of the so-named MUCOUS GLANDS OF THE STOMACH (*Schultze's Archiv*, VI. 515). He describes their different conditions in the active and inactive state, from which it would appear that during digestion the gland-cells are much richer in albumen, and that at this time the material collected in the lumen of the gland is characterized by a peculiar richness in albuminous substances.——In a paper on the GRAAFIAN FOLLICLES OF THE HUMAN OVARY, K. R. Slavjansky describes (*Virchow's Archiv*, LI. 470) the primordial follicles of the ovary, the ripe graafian follicles and the transitional forms between the two, the degeneration of the graafian vesicles without rupture and discharge of the ovum, the formation of the corpus luteum, and fatty, colloid and sclerotic changes in the parenchyma and wall of the follicle.——In his *Archiv. fur Phys.* IV. 50, E. Pflüger energetically defends his observations on the TERMINATION OF THE NERVES IN THE ACINOUS GLANDS AND IN THE LIVER, from the attacks to which they have been subjected. He gives many good reasons for considering the objects which he has described to be nerve-fibres, and not, as his opponents have insinuated, capillaries, mucous threads, or liquefied fat.

SPLEEN.—H. R. Silvester, from an investigation of the lateral homologies of the liver, stomach, and intestinal canal, considers that he has discovered the NATURE OF THE SPLEEN. (*Pamphlet*, London, 1870.) The absence of bilateral symmetry in the liver, and also in the spleen; their symmetrical situation with respect to each other; their being supplied by a corresponding trunk from the aorta, together with the want of symmetry in that portion of the abdominal viscera with which the liver is connected, all confirm, he says, the opinion that the spleen is symmetrical with the liver, and not a mesial

organ. He regards the liver as an organ similar to the spleen, combined with a biliary apparatus; when the spleen is experimentally removed, the liver can perform its function. He supposes that the absence of a biliary apparatus on the left side in connection with the spleen is due to the non-development of the small intestine on the left side; the small intestine which is developed he regards as a right intestine, the appendix vermiformis being the only representative of the left small intestine. He concludes that the spleen is a sanguiferous gland; the left lateral homologue of a portion of the liver; the liver being a combination of a sanguiferous gland, and a biliary apparatus.——E. Kyber, in a paper in *Schultze's Archiv*, VI. 540, On the STRUCTURE OF THE SPLEEN states that the capillary terminations of the arteries open directly at either right or acute angles into the commencement of the veins. Sometimes it is possible to fill the terminal twigs of the arteries by injecting from the veins, without having caused extravasation. In injecting from the arteries the material passes easily from arteries to veins, without occasioning rupture of their walls, but the larger portion of the veins has never been injected in this manner. The blood moves through the spleen only in passages with walls; the exit of corpuscles from them into the tissue is to be regarded as a pathological appearance.

MALFORMATIONS.—In the *Pathological Transactions*, XXI. 78, are recorded cases of MALFORMATION OF HEART; T. B. Peacock records two: in one the pulmonary orifice was contracted, and the aorta arose equally from both ventricles; in the other, with contraction of the pulmonary orifice, the aorta arose from the right, but communicated with the left ventricle by an aperture in the septum. In a case by H. Semple, with a patent foramen ovale and imperfect ventricular septum, the trunk of the pulmonary artery was shrivelled, and its ventricular orifice occluded; the aorta was given off from the right ventricle, and the blood found its way into the right and left pulmonary arteries through a patent ductus arteriosus. T. B. Peacock also relates the case of a girl æt. 15, where the aorta arose from both ventricles, but where the infundibular part of the right ventricle, which gives origin to the pulmonary artery, was almost entirely separated from the general sinus of the ventricle. Here also a reference may be made to two well marked cases of deficiency in the auricular septum recorded by W. W. Wagstaffe in *Path. Trans.* XIX.——In *Virchow's Archiv*, LI. 313, W. Manz describes the condition of the EYE IN THE ANENCEPHALOUS FŒTUS; and in the same volume, A. Heller relates a case of STRICTURE OF THE PULMONARY ARTERY, and one of IMPERFECT DEVELOPMENT OF THE RIGHT LOBE OF THE LIVER.——P. H. Pye-Smith discusses (*Guy's Hospital Reports*, 1870) the connection of LEFT-HANDEDNESS with transposition of the viscera and other supposed anatomical causes. He concludes that it is not to be attributed to a transposition of the viscera, or even to the transposition of the origin of the right subclavian artery to the left side of the aortic arch, but that it may be regarded as a more or less complete reversion of a condition which has now died out; and that the equal use

of both hands is a similar reversion to the primitive ambidextral condition, of which right- and left-handedness were both modifications.——John Chiene (*Pamphlet*, Edinburgh, 1870) describes a case of INTESTINAL MALPOSITION, seen in the Dissecting Rooms of the University of Edinburgh, in which the tube of the small intestine passed *in front of* the superior mesenteric artery, so that its coils lay to the left. The great intestine lay entirely to the right of the mesial plane. From the cæcum in the right iliac region the colon ran upwards to the under surface of the liver (1); it then suddenly turned downwards and forwards to reach the sacral promontory (2), when it again sharply turned upwards to the right hypochondrium (3), and then, bending downwards, passed in front of the right kidney (4), and crossed from right to left in front of the upper part of the sacrum



S. stomach. *D.* duodenum. *P.* pancreas. *s. m.* superior mesenteric artery. *G. D.* gastro-duodenal artery. *c. m.* colica media. *c. s.* colica sinistra. *c. d.* colica dextra. *i. m.* inferior mesenteric artery. *A.* aorta.

to form the sigmoid flexure and rectum. The parts of the colon marked 1, 2, 3 were firmly connected together, and to the right side of the bodies of the lumbar vertebræ, by a meso-colon 6 inches wide inferiorly, but which narrowed considerably as it ascended. The colon marked 4 was situated behind the peritoneum lining the right lumbar region. The omentum had no connection with the great intestine. The ileo-colic, right colic and middle colic arteries were

rudimentary; the left colic crossed from left to right. The author thinks that at one time the portions of the colon marked 1, 2, and 3 had each been invested by its own meso-colon, the six layers of which had ultimately become adherent to each other, and he regards the case as one in which there was a near approach to the primary condition in which the intestinal tube is joined to the spine by a continuous double fold of peritoneum; the development of a meso-colon to those parts of the gut, which usually form the ascending and transverse divisions of the colon, is in favour of the view that the omentum and transverse meso-colon are originally distinct structures.——P. H. Pye-Smith records and figures a case of RETRO-PERITONEAL HERNIA (*Guy's Hospital Reports*, 1870), where 3 feet of the jejunum were contained in a large sac, which communicated by an opening to the left of the spine, between it and the descending colon, and bounded by the inferior mesenteric artery and its left colic branch. The sac was not outside the peritoneum, but was lined continuously by serous membrane. This case resembles one described by John Chiene in this *Journal*, May 1868, and rather deserves to be called *intra-peritoneal hernia* (as was done by that observer), than *retro-peritoneal*.——Wenzel Gruber relates in *Bulletins de l'Acad. Imp. de St Pétersb.*, Sept. and Oct. 1870, cases of POLYDACTYLISM and SYNDACTYLISM. His papers contain an analysis of the literature of this branch of teratology.——C. L. Heppner contributes (*Reichert und du Bois Reymond's Archiv*, 1870, 679) an account of a case of TRUE HERMAPHRODITISM in the human subject, in which both ovaries and testicles were situated between the layers of the broad ligaments.

COMPARATIVE ANATOMY AND MORPHOLOGY.

VERTEBRATA.——To the *Proc. Zool. Soc.* 1870, J. Murie contributes the following memoirs. May 26, Notes on the ANATOMY of the PRONGBUCK. Its characters are very anomalous, in its hoofs it is allied to the giraffe, in its hair to the sheep, in its horns to the deer, in the possession of a sub-auricular skin-gland to the goat, in the presence of a gall-bladder to the bovidæ. It may perhaps be the type of a new family of Ruminants. June 9, On the SAIGA ANTELOPE. Like the Prongbuck it is difficult to fix its exact place amongst the Ruminants. "To all intents and purposes it may be regarded as an Antilopine sheep, not absolutely a sheep, but an offshoot derivative of the genus gazella rather than of the ovine antelopes, *Nemorhædus*." ——June 23, a case of VARIATION in the HORNS OF CERVUS ELDI, and on the same date an account of the mode of PROGRESSION OF PHOCA GROENLANDICA with notes on its anatomy.——In *Trans. Zool. Soc.* 1870, W. H. Flower describes the SKELETON OF DELPHINUS SINENSIS, the Chinese white Dolphin.——In XXVI., 197, 1870 of *Trans. Roy. Soc.* Edinburgh is an account by W. Turner of the ANATOMY OF BALÆNOPTERA SIBBALDII. The description is drawn up from a female with its fœtus stranded at Longniddry in 1869, and embraces an account of the external form and dimensions, colour, skin

and blubber, baleen, organs of alimentation, circulation, respiration, and genito-urinary system. The baleen is very fully described, the arrangement of the cornified epithelium of the baleen plates into tubular and cortical lamellæ, the relation to each of these groups of epithelial cells, of vascular papillæ, and the presence of vascular papillæ within the intermediate substance is pointed out, and the conclusion is drawn that the homologue of the baleen is to be looked for in the vascular folds of mucous membrane situated on the palate of so many of the mammalia. The aorta gives origin from the transverse part of the arch to a brachio-cephalic, a left carotid and a left subclavian artery. In connection with the mesenteric attachment of the gut is a remarkable tube, containing numerous diaphragms, and consisting of an alternating series of dilatations and constrictions, which give it a beaded appearance. From this tube arise the arteries which supply the walls of the intestine. This tube is in all probability a remarkable modification of the mesenteric arterial system, which, for the intestine, serves the same office that the rete mirabile does for the brain and spinal cord, to afford a mechanical arrangement by means of which the pressure from the elastic recoil of the arterial wall may be distributed and regulated before the blood enters the slender arteries within the wall of the intestine. The larynx with its curious pouch are also described, and it is stated that though there are no true vocal cords, yet that the posterior horns of the arytenoid cartilages united by the transverse ligament are so situated that they can be approximated and divaricated, and possibly act the part of vocal cords. The memoir ends with a comparison of this animal with other finners, and the conclusion is drawn that the following should be referred to the *B. Sibbaldii;* the North Berwick whale described by Knox, the Hull and Utrecht skeletons, the Gothenburg whale, Steypireyðr, Longniddry whale, and, in all probability, the Ostend whale and the "balæna tripinnis quæ maxillam inferiorem rotundam et superiore multo latiorem habuit" described by Sibbald.—— W. Turner also communicates to the Royal Society, Edinburgh, (*Proceedings* 6 Feb. 1871,) a notice of a *Sperm Whale* killed at Oban in 1829, and he refers to seven other well authenticated cases in which the sperm whale had been captured on the coast of Scotland.——W. Turner read to the Royal Soc. Edinb. (*Proceedings*, April 3, 1871) an account of the dissection of the GRAVID UTERUS and the arrangement of the FŒTAL MEMBRANES in ORCA GLADIATOR. The paper contains a description of the uterus and appendages, of the fœtal membranes, of the position and general form of the fœtus, and a comparison of its placentation with that of other mammals possessing the diffused form of placenta. The structure of the uterine mucous membrane, its subdivision into a gland layer and a crypt layer, the relations of the glands to the crypts, the structure of both, the arrangement of the vessels in connection with them, and the much greater vascularity of the crypts than the glands, are especially pointed out. The chorion, though with diffused villi, possessed not only a small bare spot at each pole, but a third larger

one opposite the os uteri internum, the non-villous portions corresponded therefore to the three uterine openings. The arrangement and structure of the villi, the relations of the vessels to them and to the chorion generally were pointed out; the plexus of capillaries within the villi becomes continuous with a capillary network situated immediately beneath the intervillous part of the chorion: this plexus is termed sub-chorionic, and from it the vein arises. Hence the blood in its passage from the terminal twigs of the umbilical artery to the rootlets of the vein flows through a complicated capillary system, one subdivision of which lies within the villi, the other between their bases. The intra-villous plexus would lie in relation to the system of capillaries lying in the walls of the uterine crypts, whilst the sub-chorionic would be in relation to the capillaries situated beneath the plane of the general uterine mucous surface. The amnion formed a continuous bag from one horn of the chorion to the other, but did not reach the poles of the latter. In the left horn, which contained the fœtus, it was only two inches, in the right nine inches from the end of the chorion; its free surface was studded with small pedunculated corpuscles. The allantois was not so extensive as the amnion. The urachus expanded into a large funnel-shaped sac, which bifurcated when it reached the chorion and formed a right and left cylindrical horn; the left reached to seven inches from the left pole of the chorion, the right to 21 inches from the right pole.——Reference may also be made to J. Murie's description of RISSO'S GRAMPUS in the last number of this *Journal*.——In *Reichert u. du Bois Reymond's Archiv*, 1870, 525, F. Krauss gives a detailed description of the OSTEOLOGY OF HALICORE DUGONG.——In *Vidensk. Selsk. Skr.* 5 *Raekke*, 9 Bd. I. nine plates, prepared by the late Prof. Eschricht, in illustration of the ANATOMY OF THE CETACEA, have been published under the editorial superintendence of J. Reinhardt. These plates illustrate points in the anatomy of *Balæna japonica* and *mysticetus*, *Megaptera boops*, *Hyperoodon latifrons* and *rostratus*, *Delphinapterus leucas* and *Phocæna communis*.——An elaborate and beautifully illustrated memoir, by R. Owen, on DIPROTODON AUSTRALIS, is in *Phil. Trans.* 1870; and in *Trans. Zool. Soc.* 1870, are two memoirs on DINORNIS.——A. Nuhn describes (*Reichert u. du Bois Reymond's Archiv*, 1870, 333) the FORMS OF THE STOMACH IN THE VERTEBRATA. In connection with differences in size and form the following points are to be considered: the amount required for nutrition; the digestibility and quantity of the nutritive material; the form and size of the cavity of the body in which the stomach is situated; arrangements which increase the action of the gastric juice on the nutritive materials; the assumption by the stomach of functions which it is customary for other organs to perform, as in birds, where the stomach is also a masticatory apparatus.——To *Reichert u. du Bois Reymond's Archiv*, 1870, 437, R. Himstedt contributes an account of some peculiarities in the CRANIAL BONES OF LEPUS, with observations on the organ of hearing.——An elaborate memoir on the STRUCTURE OF THE WING OF THE BAT, by Jos. Schöbl, is in *Schultze's Archiv*, VII. 1. The layers of the skin, the hairs and their glands, the

elastic bands, the transversely striped muscles, the blood-vessels, nerves and terminal bodies are carefully described.

J. B. Pettigrew communicates to *Proc. Roy. Soc. Edinb.* Jan. 16, 1871, an abstract of a memoir ON THE PHYSIOLOGY OF WINGS. The wing is a screw structurally and functionally; the blur or impression produced on the eye by the rapidly oscillating wing is twisted on itself and resembles the blade of an ordinary screw propeller. The twisted configuration of the wing and its screwing action are due to the presence of figure of 8 looped curves on its anterior and posterior margins. If the wing is in one piece it is made to vibrate figure of 8 fashion in a more or less horizontal direction; it attacks the air by a series of zigzags. If the wing is in more than one piece it is made to oscillate in a more or less vertical direction. The wing is closed and its area diminished during the up stroke, expressly to avoid the resistance of the air. The wing can produce artificial currents and utilise and avoid natural currents; the down and up strokes form one continuous act, and neither is complete without the other. The wing balances the body, in consequence of its travelling at such a speed as enables it to convert the area mapped out by its vibrations into what is practically a solid basis of support. The wing, whether in one or many pieces, rotates on two centres, the one corresponding to the root of the wing (short axis), the other to the anterior margin (long axis). Weight is necessary to the flight of the insect, bat, and bird. In aërial flight the under or concave surface of the wing is applied from above; in sub-aquatic flight from below. Weight contributes to horizontal flight, for a flying animal when it drops from a height with expanded motionless wings falls downwards and forwards, the wings converting the vertical fall partly into forward travel. The author enters at length into the consideration of the principles on which artificial wings should be constructed.——Hugo Magnus gives in *Siebold und Kölliker's Zeitsch.* XXI. 1 a memoir on the structure of the OSSEOUS CRANIUM OF BIRDS.——To *Jenaische Zeitsch.* VI. Part 2, Carl Gegenbaur contributes a memoir on the PELVIS OF BIRDS. In the first place he describes the form of the pelvis in different birds. Then he compares the various portions of the pelvis in the different classes of vertebrata, and determines which are to be regarded as sacral vertebræ, not only by a consideration of the form of the bones, but from the arrangement of the spinal nerves. In employing the arrangement of the nerves as an aid in the determination of the morphology of the bones he is carrying out that principle which John Goodsir had previously employed (*Anatomical Memoirs*, II.) in the determination of the morphological constitution of the limbs and other parts of the body. He shows that in reptiles (crocodiles) the sacral ribs are fixed to the body and transverse process of the vertebræ by their proximal ends; the distal end carries the ilium. In birds the proximal ends of the sacral ribs are fixed to the vertebral body; the distal end is united with that of the transverse process, the two together supporting the ilium. The foramen transversarium is between the rib and transverse process. In mammals (man) the proximal end of the sacral rib is united to

the vertebral body and arch; the end of the rib alone goes to the ilium. In comparing the ilium of birds with that of mammals he shows that the homologue of the crista ilii is the hinder border of the post-acetabular portion of the pelvis in birds; the præ-acetabular part is nothing but an outgrowth forwards of that part of the bone. ——R. O. Cunningham in *Proc. Zool. Soc.* May 12, 1870, records some observations on the ANATOMY OF KINGFISHERS. In *Ceryle stellata* he found that the biventer cervicis muscle was united to its fellow by a narrow but strong transverse tendon, and that, in addition, a strong membrano-tendinous junction connected them together at their insertion into the occiput. Similar connecting bands were not found in *Alcedo ispida* and *Dacelo gigas.*——St George Mivart contributes to *Proc. Zool. Soc.* April 28, 1870, an account of the AXIAL SKELETON OF URODELA.——George Gulliver again directs attention (*Proc. Zool. Soc.* May 12, 1870) to the importance of the study of the MUSCULAR SHEATH of the ŒSOPHAGUS in connection with the classification of the vertebrata.——W. K. Parker communicates (*P. R. S. Lond.* Jan. 19, 1871) an abstract of a memoir on the STRUCTURE and DEVELOPMENT of the SKULL of the COMMON FROG. He concludes that the skull of the adult is composed of: 1st, its own proper membranous sac; 2nd, of a posterior part which is a continuation, in an unsegmented form, of the vertebral column; 3rd, of laminæ which grow upwards from the first pair of facial arches, and which enclose the fore-part of the membranous sac, just as the investing mass of the cranial part of the noto-chord invests the hinder-part; 4th, the ear sacs and the olfactory labyrinth become inextricably combined with the outer case of the brain; 5th, the subcutaneous tissue of the scalp becomes ossified in certain definite patches, which are the cranial roof-bones. The proper facial bars are: 1st pair, trabeculæ; 2nd pair, mandibular arch; 3rd pair, hyoid arch; 4th to 7th pairs, branchials. The metamorphoses of the cartilaginous rods which form the facial arches have been traced. The anterior two-thirds of the cranium is developed by out-growing laminæ from the trabeculæ, which after a time become fused with the posterior or vertebral part of the skull.——Jos. Oellacher describes (*Schultze's Archiv*, VII. 157) the EARLY DEVELOPMENT OF THE HEART AND PERICARDIUM in *bufo cinereus.*——Ramsay H. Traquair communicated to the *Geol. Soc. of Ireland*, June 8, 1870, notes on the EXTERNAL CHARACTERS OF CALAMOICHTHYS CALABARICUS, a West-African ganoid. The head, the body scales, the lateral line, and the fins are described in this communication.——P. J. van Beneden has published in *Mém. de l'Acad. Roy. des Sc. de Belgique*, 1870, a memoir on the FISH which frequent the coasts of Belgium, their food, their PARASITES and COMMENSALS. This memoir will be found most useful as furnishing in a concise form a list of the numerous parasites and commensals with which fish are infested.—— An abstract of a memoir by A. Günther on CERATODUS, a new genus of ganoid fish from Queensland, is in *P. R. S. Lond.* March 16, 1871. He considers that two species exist, *C. forsteri* and *C. miolepis.* "Ceratodus and Lepidosiren are more nearly allied to each other

than to any third living fish; they are well marked modifications of the same Dipnoous type, the latter genus diverging more towards the Amphibians than the former."——C. B. Reichert furnishes to his and *du Bois Reymond's Archiv*, 1870, 755, a short account of the anatomy of BRANCHIOSTOMA LUBRICUM.

INVERTEBRATA.—G. J. Allman contributes to *Trans. Roy. Soc. Edinb.* (16 May, 1870), a memoir on the GENETIC SUCCESSION OF ZOOIDS in the HYDROIDA, in which he expresses, in the shape of formulæ, the alternation of sexual with non-sexual development. If t be the trophosome or entire assemblage of nutritive zooids in a colony and g the gonosome or generative zooids, then $t+g \times t+g \times \ldots$ &c. will be the general expression for the genetic succession in the life of the hydroid, + indicating succession by zooidal, × by embryonal development. Other formulæ are also given to express the binary, ternary, and quaternary types of hetero-morphism, seen respectively in *Corymorpha*, *Dicoryne*, and *Campanularia*. As the hydranth, or proper nutritive zooid, does in almost every instance, either directly or through the medium of the common basis, repeat itself indefinitely by budding before the time arrives when an element of the gonosome is to be budded off, a series of homo-morphic zooids may introduce themselves into the hetero-morphic succession. From a comparison which the author institutes between the gonosome of the hydroida and the inflorescence of plants, the conclusion is drawn that when, in the hydroida, the generative buds are borne upon a special gonosomal axis, like the flowers in an inflorescence, the order of succession is far more frequently centrifugal than centripetal, which is the opposite of what prevails in plants.—In *Siebold u. Kolliker's Zeitschrift* XXI., E. Claparède makes contributions to the ANATOMY and DEVELOPMENT of the MARINE BRYOZOA; and W. von Nathusius a paper on the CAPSULE of the EGG of the ringed snake, &c.——In *Reichert u. du Bois Reymond's Archiv*, 1870, 366, A. Stuart gives an account of the MEDUSA of VELELLA; and on p. 660, L. Stieda relates the structure of POLYSTOMUM INTEGERRIMUM.——W. C. McIntosh (*Trans. Roy. Soc. Edin.* 1870) records some points in the STRUCTURE OF TUBIFEX. The external form and the arrangement of the bristles, the body-wall, the perivisceral fluid and corpuscles, the digestive apparatus, the circulatory and generative systems are all considered.——E. Ray Lankester makes observations on the ORGANIZATION of OLIGO-CHŒTOUS ANNELIDS (*Ann. Nat. Hist.*, Feb. 1871). He suggests a modification of the group-subdivisions of these annelids proposed by Claparède, and directs attention to a new Thames worm, *Tubifex umbellifer*. He describes the setæ, endothelium of the perivisceral cavity, generative organs of *Tubifex*, and the genital organs of *Chætogaster* and *Nais*.——In *Month. Micros. Journ.*, Feb. 1871, B. P. Lowne records his Observations on the ANATOMY OF ASCARIS LUMBRICOIDES.——Jas. Murie describes a species of TÆNIA FROM THE RHINOCEROS, which he considers to be, in all probability, new, *Proc. Zool. Soc.*, June 23, 1870.——In *Verhand. der Phys-med. Gesellschaft zu Würzburg*, 1870, appear contributions by A. Kölliker to our knowledge of the POLYPS. He describes *So-*

landeria, a new genus *Semperina rubra*, and *Pseudo-gorgia Godeffroyi*. ——R. Hartmann continues his observations on the PARASITIC CRUSTACEA (*Reichert u. du Bois Reymond's Archiv*, 1870, 726). The subject of his present paper is a new species, which he names *Lernæocera Barnimii*. It is a parasite on the *Labeo niloticus*.——W. Dönitz makes an adverse criticism (*Reichert u. du Bois Reymond's Archiv*, 1870, 761) on the views expressed by Kowalevsky and Kupffer (see *Reports* III. 205; IV. 307) as to the nature of the so-named CHORDA DORSALIS in the LARVAL ASCIDIAN, and the presumed relationship which it indicates between the invertebrates and vertebrates. His observations were conducted on *Clavelina lepadiformis*, and he believes that none of Kowalevsky's positions are satisfactory. The formation of the intestine, the body cavity, the nervous axis, and the axial rod are, he says, in the tail of the ascidian larva quite different from the development of the same organs in the vertebrate embryo, and their signification has been completely misunderstood by that observer. Here also it may be stated that E. Metschnikoff had, in *Bul. de l'Acad. Imp. de St Pétersb.* XIII. 1869, 293, maintained that there was no analogy between the development of the ascidians and the vertebrata.——A. Kowalevsky contributes (*Schultze's Archiv*, VII. 101, 1871) additional observations on the DEVELOPMENT of the SIMPLE ASCIDIA. He gives numerous figures, with descriptions, of the development of the free-swimming larva of A. mammillata, and considers, from what he has seen, that scarcely any one can doubt that the chorda of the ascidian is both analogous and homologous with the chorda of the vertebrate.

REPORT ON THE PROGRESS OF PHYSIOLOGY, from 1st January to 1st April, 1871. By THOMAS R. FRASER, M.D., F.R.S.E., F.R.C.P.E., *Lecturer on Materia Medica and Therapeutics, Edinburgh*, T. LAUDER BRUNTON, M.D., D.Sc., *Lecturer on Materia Medica at the Middlesex Hospital, London*, and DAVID FERRIER, M.A., M.D., *Demonstrator of Practical Physiology, King's College, London.*

DR FRASER'S REPORT.

Physiological Action of Medicinal and Poisonous Substances.

NITROUS OXIDE.—Dr Maclaren (*Edin. Med. Journal*, Jan. 1871, p. 591) thus describes the phenomena usually observed when nitrous oxide gas is inhaled:—After two or three inspirations there is an increase in the force and frequency of the pulsations. In about twenty seconds the breathing is also increased in frequency, if it has been originally steady and regular; frequently, however, owing to the nervous condition of the patient, the respirations are from the commencement irregular, shallow and rapid. In about thirty seconds the patient's colour begins to turn livid. When the inhalation has been continued for about one minute, the pulse is almost invariably observed to fall in force and frequency, and the breathing is often laboured and even stertorous. In the majority of cases anæsthesia occurs in one minute and twenty seconds. It is recognised by various signs; the one least open to mistake is the sudden change in the appearance of the patient, a change difficult to describe, but readily recognised; another sign is the twitching of the hands. It is not by any means necessary to carry the administration to this extreme in every case, but when twitching of the hands is present for a few seconds, the patient is quite insensible. There often occurs considerable muscular rigidity during the later stages of the administration, rendering it advisable in cases of dentistry to insert a plug between the teeth before commencing the administration. Dr Maclaren draws attention to a curious phenomenon apparently unnoticed by previous observers, namely, the retention of the sense of hearing during suspension of all the other senses. He further points out that the same dependence cannot be placed on the insensibility of the conjunctiva to touch as in chloroform inhalation, for he has observed that patients will close the eye when it is touched while they are quite insensible to pain, and that they will sometimes keep the eye steadily open while conscious. After the administration, recovery is usually very rapid and complete, the patient waking up as from a sleep. It sometimes, however, happens, especially in

[1] In order to assist in making this Report as complete as possible, the authors will be glad to receive copies of original contributions to physiology. Papers on the *Physiological Action of Medicinal and Poisonous Substances* to be sent to Dr Fraser, the University, Edinburgh. Those on *Physiological Chemistry* to Dr Gamgee, Royal College of Surgeons, Edinburgh. Those on other physiological subjects to Dr Brunton, 23, Davies Street, London, W.

We much regret that Dr Gamgee is prevented by illness from arranging and completing his Report on PHYSIOLOGICAL CHEMISTRY for the present number.

young ladies between the ages of fourteen and twenty, that there is an intermediate stage between that of complete unconsciousness and recovery, when some excitement is exhibited, such as tossing about restlessly in the chair and weeping.——Dr Amory of the United States has made a number of experiments for the purpose of determining the influence of this gas on the respiration and circulation of the blood (*New York Medical Journal*, August, 1870, and Pamphlet). He states that during the anæsthesia caused by nitrous oxide the amount of carbonic acid exhaled is diminished by about one-half, while the pulse is accelerated and weakened. Though the gas freely enters the lungs, it does not part with any of its oxygen to the blood, nor cause the elimination of as much carbonic acid from the blood as when atmospheric air or pure oxygen is respired. Accordingly, oxidation of the venous blood ceases and stagnation occurs in the capillaries. Dr Amory explains the production of anæsthesia by the oxidation of the tissues being insufficient.—Although properly belonging to the subject of practical therapeutics, the following general deductions, made by Dr Holden from a large series of clinical observations, are of sufficient interest to be included in this Report (*The American Journal of the Medical Sciences*, July, 1870, p. 61):—"1st. Inhalation of nitrous oxide is in some degree likely to prove injurious in cases of phthisis. 2nd. Inasmuch as pulmonary congestion is almost a necessity to anæsthesia by this agent, and is concomitant with a well-marked tendency to increase of hæmorrhage from any cut or abraded surface, its inhalation is somewhat hazardous in cases where hæmoptysis has occurred, or where there exists an hæmorrhagic diathesis. 3rd. The sense of relief frequently experienced by those having diseased lungs is alluring simply and does not indicate benefit; and believing it to be due to an annulling of hyperæsthesia of the bronchial nerves, the inference follows that the agent may prove curative or at least palliative in asthma and in affections accompanied by bronchial spasm."

Chloral Chloroform and Ether, subcutaneously administered.—Doubts have been expressed in various quarters as to the possibility of producing narcosis by the subcutaneous injection of chloroform or ether. Dr Benjamin Richardson has already shown that the ordinary physiological effects of chloroform may be induced by this mode of administration, and in confirmation of this, Dr Weir Mitchell (*American Journal of the Med. Sciences*, April, 1870, p. 394) has exhibited, before the College of Physicians of Philadelphia, a pigeon narcotised by the subcutaneous injection of chloroform. At the same time he exhibited a pigeon which had received in the same way two doses of ether, the first of twenty-five, and the second of thirty drops, by which profound and long-continued sleep was produced. Contrasting the effects caused by the subcutaneous injection of chloral and chloroform, he found that the dose of chloroform required to produce sleep was greater than could be formed by the decomposition of a dose of chloral amply sufficient to produce the same effect.

NITRITE OF AMYL.—Dr Goodhart (*The Practitioner*, Jan. 1871, p. 12) has examined the symptoms produced on himself by the inhalation of from three to ten minims of nitrite of amyl. The most interesting points observed may be thus summarised:—In all cases the circulation was first affected, an increase occurring in the pulse-beats in from three to ten seconds, after which flushing of the face appeared. Sphygmographic tracings showed that in the first place, and coincidently with the great acceleration of the heart's action, the up-stroke was almost imperceptible, after which it became sudden and jerking. The respirations were not altered in frequency; but when the face became flushed, great inclination to cough was experienced along with an indescribable feeling of fulness about the chest. Besides these phenomena, haziness of vision was observed, and, among the after effects, dull-aching in the head and general lassitude.

HYDROCYANIC ACID.—*Is it present in the smoke of tobacco?* In order to determine whether M. Vogel's affirmative answer to the above question is true, MM. Poggiale and Marty (*Journal de Pharmacie et de Chimie*, IV. *Série*, T. XI. 1870, p. 216) have conducted a careful and elaborate investigation. They conclude:—(1) That the smoke of tobacco does not contain the least trace of hydrocyanic acid; and (2) that Schœnbein's reaction, which was trusted to by M. Vogel, is fallacious.

CLARET. In the last number of this *Journal* (p. 201) a full abstract was given of an important investigation by Professor Parkes and Count Cyprian Wollowicz, M.D., on the action of alcohol on the healthy human body. A paper has been published by these observers, (*Proc. R. S.* No. 123, June 16, 1870, p. 73), in which similar observations are described regarding the effects of claret on the same individual. The wine used was of good quality, of the second growth of the Haut Brion vintage of 1863, and was sold in London at the price of 60*s.* per dozen. The experiments were continued for 30 days, the man having abstained from any alcoholic beverage for 16 days previously. During the first 10 days, water only was taken at dinner, during the next 10 days, the wine was substituted for the water; 10 fluid ounces (284 cub. centims) being given on the first five days, and 20 fluid ounces (568 cub. centims) on the last five days. The wine was taken at dinner-time, at a quarter past one o'clock. In the last 10 days water was again given. Only two circumstances (except the taking of the wine) were different in this second series of experiments as compared with the first. The first series was made in February and March, 1870, when the weather was very cold; the present in May and June in very hot and dry weather. The only influence traceable to this altered condition of climate was, that the amount of water allowed proved to be insufficient, and the man suffered some discomfort from thirst. The other alteration was, that the man gained 4lbs. in weight, and was still gaining a little when the experiments were commenced; he continued to do so slowly until the twenty-fourth day, when his health began to give way and he lost weight. The general results of these experiments are in all

respects identical with those with alcohol and brandy, that is to say, there was a marked effect on the heart, coinciding tolerably well in amount with the effect produced by pure alcohol in the former experiments; there was no unequivocal alteration of temperature in the axilla or rectum; no alteration in the elimination of nitrogen; no alteration in the phosphoric acid of the urine; no alteration in the alvine discharges. In other words, claret-wine in the above quantities cannot so far be distinguished from pure alcohol. Its most marked effect, the increase of the heart's action, must be ascribed to the alcohol in great measure, though the ethers may play some slight part. The authors carefully guard against this statement conveying the impression that the dietetic effects of red Bordeaux wine and of dilute alcohol are identical. They point out that the differences between them must be sought in their effects on primary digestion and assimilation, delicate and subtile influences, which experiments, like those recorded in the paper, do not touch. The influence of the sugar, of the salts, and of the acidity, must also be appreciated by other methods. By these experiments they consider themselves better able than by those previously made to define what might be considered moderation for this man. The ten ounces of wine, containing about one fluid ounce of pure alcohol, did not cause the least unpleasant feeling of heat or flushing. The 20 ounces, containing almost two fluid ounces of alcohol, were manifestly too much. He felt hot and uncomfortable, was flushed, the face was somewhat congested, and he was a little drowsy. Moreover, alcohol then began to appear in the urine. Therefore he ought certainly not to take more than one ounce of absolute alcohol in 24 hours.

TURPENTINE AS AN ANTIDOTE TO PHOSPHORUS-POISONING. Several years ago, Mr Personne propounded the theory that the poisonous effects of phosphorus are solely due to its action in consuming the oxygen of the blood, and that oil of turpentine acts as an antidote by preventing this consumption. The truth of these theories has now been called in question by MM. Currie, D.M. and P. Vigier (*Journal de Pharmacie et de Chimie*, IV. Série, T. XI., 1870, p. 63). Experiments were made on dogs and rabbits to test the power of the antidote, but all of them were unsuccessful. The inability of rabbits to vomit rendered them peculiarly serviceable, as an important source of fallacy, difficult to overcome in dogs, was thereby avoided. The general result arrived at was, that the poisonous action of phosphorus is not prevented, in dogs or rabbits, by oil of turpentine. It is pointed out that Personne's theory of the action of phosphorus is untenable, when it is considered that eight milligrammes of phosphorus is sufficient to kill a rabbit weighing three kilogrammes, while the quantity of oxygen which this dose is capable of abstracting from the blood is only one centigramme. It is irrational to suppose that the abstraction of so minute a quantity of oxygen from the blood of a full-grown rabbit would result in death by asphyxia.

MEMORDICA ELATERIUM.—The researches of Zwicke, Hagentorn,

Untiedt, and Bastgen have shown that convolvulin, elaterin, and other neutral purgatives, are unable to influence the evacuation of fæces unless they are brought into contact with the bile. Dr Köhler *Virchow's Archiv*, IV. hft. 3, 1870) confirms these observations, and also advances valuable proof in support of the opinion that the inactivity of these principles is due to their insolubility in nearly all the fluids of the body excepting the bile. In the latter, solution is effected by a compound being formed between the drastic principles and glycocholate or taurocholate of soda; the taurin and choloidinate of soda in the bile being unable to produce a soluble substance with these neutral principles. It is further shown that a similar transformation may be produced by soap. Dr Köhler proposes to divide the vegetable cathartics into two great divisions: the first including those which are active independently of any change produced in them by bile, such as senna, rhubarb, croton oil, and aloes; and the second, those which cannot exert any cathartic influence until they are modified by the bile. The latter class of purgatives may be subdivided into those whose action is a purely local one, as scammony, gamboge, and jalap, and those whose action is constitutional as well as local, as colocynth, elaterium, &c.

STRYCHNIA.—An elaborate analysis of 143 cases of poisoning with strychnia is published by Dr J. St Clair Gray (*Glasgow Med. Journal*, Feb. 1871, p. 167), along with some interesting observations on its mode of action, and the description of a number of experiments with various antidotes. In reference to the last subject, Dr Gray has examined the value of curara, chloroform, physostigma, and nitrate of amyl, but his trials have proved unsatisfactory. Only some slight evidence of a power of counteraction was obtained, which was rather more marked with nitrate of amyl than with any of the others. When this substance was used, four out of ten rabbits recovered after the administration of strychnia; but, unfortunately, no evidence is given to show that fatal doses of the poison were administered.

AKAZGA.—MM. Rabuteau et Peyre have recently examined the physiological action of the akazga or icaja ordeal poison of the Gaboon (*Comptes Rendus*, 8 Aôut, 1870, p. 353), by experiments on frogs, rabbits, and dogs. The general conclusion to which they have arrived is, that the action is a tetanic one, possessing a greater similarity to that of brucia than of strychnia (MM. Rabuteau et Peyre refer to a previous research with this poison by MM. Pécholier et Saintpierre, but they have overlooked the researches of the Author of this Report, published in the *Proc. R. S. E.*, April 1867; in the *Brit. and For. Med. Ch. Rev.*, July, 1867, and in *Trans. of the Botanical Society of Edinburgh*, 1867—68. The former of these papers gives a full description of the action of akazga, and details of a process by which the active principle may be obtained in the form of a crystalline alkaloid; whilst the last paper contains a comparison of the microscopic structure of its stem with that of *Strychnos Nux-vomica*).

ATROPIA.—Dr H. C. Wood, Jun. contributes some interesting facts to our knowledge of the physiological action of Atropia on birds (*American Journal of Med. Sc.*, January, 1871, 128). He finds that atropia even in strong solution when applied to the eyeball of a pigeon has no action on the pupil. The iris also remains unaffected when large toxic doses are given by the stomach or subcutaneously. Being somewhat astonished with the comparatively enormous doses required to produce any symptom in pigeons, Dr Wood made a number of experiments to determine the minimum fatal dose. In the first place, he found that fifteen grains of extract of belladonna had no effect when introduced into the stomach of a pigeon; but as this immunity might be due to rejection of a portion of the poison by the vomiting that occurred, six grains of sulphate of atropia was similarly administered, and still no marked effect was observed, although the pigeon retained this enormous dose. Experiments were then made in which sulphate of atropia was administered by subcutaneous injection, which proved that a dose of at least two grains is required to kill an ordinary robust adult pigeon. Dr Wood refers this insusceptibility to (1) absolute obtuseness of the nerve-centres of pigeons to the action of atropia, (2) very rapid elimination of the poison, and (3) slowness of its absorption.

ACTION OF CERTAIN DIURETICS: (*Citrate and acetate of potash, Spiritus Ætheris Nitrosi, and Oil of Juniper*).—From a number of carefully devised experiments made on himself, Dr Nunneley (*Med. Ch. Trans.*, 2nd Series, XXXV. 1870, p. 31) arrives at the following generalizations:—*that in health*. 1. Citrate and acetate of potash only slightly increase the quantity of water excreted by the kidneys. 2. They distinctly lessen the amount both of urea and of solids excreted. 3. Spiritus ætheris nitrosi slightly increases the amount of urinary water. 4. It decidedly reduces the quantity both of the urea and solids. 5. Oil of Juniper slightly reduces the amount of water excreted. 6. It appreciably increases both the urea and solids. It would thus appear that of these four medicines, undoubtedly shewn by clinical evidence to possess a diuretic action in various pathological conditions, citrate and acetate of potash and spirit of nitrous ether actually reduce the urinary solids, whilst they slightly increase the water, and oil of juniper increases the solids while it lessens the water, in a state of health.

ANTAGONISM BETWEEN MORPHIA AND HYDROCYANIC ACID.—Judging from the results of eight experiments performed on dogs, Professor Reese of the University of Pennsylvania (*American Journal of Med. Sc.*, Jan. 1871, p. 133) believes that he is warranted in concluding "that the antagonism between prussic acid and morphia is very slight, if indeed it exists at all." (With great deference to the opinion of an observer occupying so distinguished a position as Dr Reese, we would take exception to his statement that "in order to get a clear idea of this real or supposed antagonism, it will be necessary, first, to understand precisely what are the symptoms pro-

duced by each of these poisons when taken separately." Such a knowledge can be of only slight value. The same symptoms (using the word, as Dr Reese does, to imply the obvious general appearances produced) may be caused by different actions, and it would be absurd to imagine that one substance will antagonise another merely because it produces apparently contrary or opposite symptoms. One poison may paralyse the heart by stimulating the vagi nerves, another by destroying or suspending the functional activity of the cardiac ganglia, and yet the symptoms produced by both, so far as the heart is concerned, might be exactly the same. Nevertheless it would be improper to expect that a substance which impedes the conductivity, or causes paralysis, of the vagi nerves, would antagonise both of these poisons.)

THE EFFECTS OF POISONS ON FROGS DEPRIVED OF BLOOD.—An ingenious method of "localising" the action of certain poisons has been pursued in an investigation by Dr Lewisson, of Berlin, (*Glasgow Med. Journal*, Nov. 1870; from Reichert and du Bois Reymond's *Archiv*, 1870). He takes advantage of the fact pointed out by Cohnheim, that in frogs a solution of common salt can be injected into the vessels, so as nearly completely to wash out the blood, notwithstanding which the heart continues to beat vigorously, and the power of voluntary motion is to a considerable extent retained. If the action of a neurotic poison be manifested in such frogs, it is apparent that it must influence the nervous system directly, and not indirectly through a primary action on the blood. Dr Lewisson finds that such neurotics as morphia, chloroform, &c., which produce their effects rapidly in normal frogs, do so likewise in the "salt-frogs;" and from this he concludes that they act directly upon the nerve-substance. On the other hand, such substances as hydrogen, carbonic acid, carbonic oxide, &c., produce their effects by a primary action upon the blood.

VENOM OF THE SCORPION.—A memoir on this subject by M. Jousset was presented to the Paris Academy of Sciences by M. Claude Bernard (*Arch. Gén. de Méd.*, Oct. Nov. and Déc. 1870, p. 572). From a large number of experiments, M. Jousset concludes that the venom of *scorpio occitanus* acts directly and solely on the red corpuscles of the blood; the nature of the action being a destruction of their power to glide over each other, from which it results that small masses of adhering corpuscles are formed which completely obstruct the capillaries. A certain quantity of the venom produces this modification in a definite number of the corpuscles, and the symptoms of poisoning are directly proportional to the quantity of modified corpuscles present in the blood. There is, therefore, a broad distinction to be drawn between the nature of the action of this venom and of those animal poisons which resemble ferments.

VENOM OF THE RATTLESNAKE.—The chief portion of a paper by Dr Weir Mitchell on poisoning with rattlesnake venom (*American Journal of Med. Sciences*, April, 1870, p. 317), is occupied with a

report on the microscopic character of the blood by Dr Jos. G. Richardson. This description confirms Dr Mitchell's previously expressed, opinion that the peculiar corpuscles described by Dr Halford are merely modified leucocytes. Similar corpuscles were spontaneously produced in the blood of animals which had not been poisoned, after intervals of twenty or thirty hours; and it is suggested that they are in reality only white-blood globules which had undergone an alteration analogous to that produced by reducing the specific gravity of the blood.

DR BRUNTON'S AND DR FERRIER'S REPORT.

Nervous System.

EXCITABILITY OF THE CEREBRUM.—Fritzsch and Hitzig (*Reichert and Dubois' Archiv*, 1870, 300) publish some very interesting results of experiments on dogs with reference to the question of the electric excitability of the Cerebrum. Their experiments were determined by the observation of Hitzig, that when a constant current is passed through the occiput or the temples movements of the eyes are produced. They laid bare the brain by trepanning the skull. They employed as an irritant a constant current of minimum strength. In the anterior part of the convexities of the hemispheres they found certain definite regions, irritation of which caused movements of certain groups of muscles on the opposite side of the body. The irritation proceeded principally if not exclusively from the positive pole. Tetanic irritation of the centres caused tonic spasms of the muscles, which subsided gradually. Weak induction currents caused after-movements of an epileptic character. The results are not due to irritation of the basal ganglia or individual nerves, as shewn by the weakness of the currents, the occurrence of the contractions on the opposite side of the body, the entire cessation of the contractions on great hæmorrhage, which causes the irritability of the brain to sink rapidly, while that of nerve and muscle still remains, or even becomes exalted, &c. The removal of these localised centres does not cause complete paralysis of the muscles governed by them, but merely a blunted sense of the locality of the limb. Hence these centres are not the only centres concerned in the movements of the muscles of the limbs.

SENSIBILITY OF POSTERIOR ROOTS.—G. Giannuzzi (*Revista Scient.* 1870, 114, *and Centralb. d. Med. Wiss.* 1870, 622) found that when the posterior roots of several sacral and coccygeal nerves were divided, the central end of the uppermost remained slightly irritable for several months. On microscopic examination he found that the greater part was degenerated, but a few fibres remained completely normal. Whence these came he could not say, but did not think they arose from the nerve above, which had not been divided.

RAPIDITY OF CONDUCTION IN MOTOR NERVES.—Helmholtz and Baxt (*Monatsbericht der Berlin Acad.* 1870, 184) in their new researches on the rapidity of conduction in motor nerves use the same method as before, the arm being immovably fixed in plaster of Paris, and the median nerve irritated first above the elbow and then at the wrist. The contractions which are thus produced in the muscles of the ball of the thumb are registered by Marey's myograph. Instead of using a revolving cylinder the curves were registered by Fick's plan on a glass plate attached to the bob of a pendulum, which in the middle of its swing caused a spark from an induction coil to irritate the nerve. They found that a higher temperature increases the rapidity of conduction in human nerves as well as those of the frog. Conduction is quicker in the upper than in the forearm, and this seems to depend on unequal conduction in the nerves themselves independently of any difference between the temperature of the parts. When the nerve was irritated by shocks quickly following one another, the contraction of the muscle was no more powerful than when only one was employed, unless an interval of more than $\frac{1}{360}$th of a second intervened between them. Constant currents, especially descending ones, readily produced tetanus in the muscle, during which oscillations lasting ·09 of a second took place in it.

Place (*Pflüger's Arch.* III. 424) in a research undertaken along with Van West, found by Helmholtz's method the rapidity of conduction in motor nerves to be 50—60 metres in a second, the mean being 53, a much greater rapidity than Helmholtz had found in motor nerves (33 metres), and very nearly the same as that he had found in sensory ones. When the irritation was applied high up, near the coraco-brachialis, they found a rapidity of 35·26 metres. The rapidity of conduction in the forearm they found to be much greater than in the upper arm, it being 56—62 metres in the former and only 12—14 in the upper.

IRRITABILITY AND RAPIDITY OF CONDUCTION IN NERVES DURING ELECTROTONUS.—Wundt (*Pflüger's Arch.* III. 437) found that when a constant current is passed through a nerve the irritability at first increases gradually throughout the whole nerve, till at the positive pole or anode it reaches a certain maximum, and then sinks below the normal (anelectrotonus), while at the negative it is increased all the time the current passes (catelectrotonus). At the anode the increased excitability remains for a certain time after the contraction of the attached muscle, caused by closure of the current, has ceased. The stronger the current used the longer does the increase of irritability continue, and it remains longest in the parts of nerve furthest from the electrodes. Even when the current is so weak as to cause no muscular contraction, it can increase the irritability of the nerve, and an irritant applied to it causes the muscular contraction to last a longer time. When strong currents are applied, the increased irritability is shown not only by longer duration of the muscular contraction, but by its greater height and shorter latent period. The farther from the muscle the irritation is applied, the greater the certainty

with which the increase in height and duration of the contraction can be observed. Shortening of the latent period is only noticed with certainty, when the poles of the induced current by which the nerve is irritated, are applied between the muscle and the poles of the constant current by which the nerve is polarized. The period of latent irritation becomes shorter as the strength of the irritation is increased, but only if the irritating current is a descending one, or lasts for such a short time that electrotonus cannot be induced by it. If it be an ascending one and last long enough to induce electrotonus, the resistance to the passage of irritation which is produced at its anode greatly increases the latent period, at the same time that it diminishes the height and duration of the curve of contraction, but it does not diminish them in the same proportion. Strong currents, then, produce slighter contractions than weaker ones, and contractions of the same height and duration may be produced by a strong and weak current, but they differ in the length of the latent period, which is greater when a strong current is employed.

IRRITATION OF NERVES BY VARIATIONS IN THE STRENGTH OF A CONSTANT CURRENT.—Nasse (*Pflüger's Arch.* III. 476) in a series of experiments made to determine the amount of sudden variation in a constant current of different strength necessary to produce a contraction in muscle, found that as the current was made gradually stronger the amount of sudden increase required became less and less till it reached a minimum and then began to increase, so that with a strong current a great increase was necessary. The necessary amount of sudden diminution however, increased constantly with the strength of the current, but more slowly at first than afterwards.

THE ELECTRO-THERAPEUTIC AND PHYSIOLOGICAL METHODS OF STIMULATION.—W. Filehne (*Deutsch. Archiv für Klin. Med.* VII. 575—586) compares the results of electro-therapeutic stimulation, according to Brenner's polar method, with the results of physiological stimulation of the dissected nerves of the frog. His method was, to place one electrode on the sciatic nerve, and the other on muscles of the thigh. He found that the results corresponded with those obtained by Brenner in man, viz. that stimulation proceeds from the cathode in the ascending current, and from the anode in the descending current, according to Pflüger's law. Stimulation with one electrode is to be accounted for as if the other electrode were placed simultaneously above or below (central and peripheric) the spot stimulated by one electrode. In order to ascertain if also with strong currents electro-therapeutic and physiological experiments agreed, the author made experiments on frogs and rabbits, and found that the results corresponded also in mammals with the height of the third series of curves as given by Pflüger. When both electrodes are placed on a nerve, in *weak* currents, the irritation proceeds from the central electrode, as Brenner states; in *strong* currents, it is from the distal electrode. In sensory nerves stimulation by the cathode appears to be compounded of a peripheric ascending and a central descending

current. In the polar method the action of one and the same electrode is the same in both classes of nerves.

Eye.

THE INFLUENCE OF THE SYMPATHETIC ON THE EYE.—Dr Sinitzin (*Centralblatt*, No. 11, 1871) records some very interesting results which follow extirpation of the superior cervical ganglion of the sympathetic. He finds that the cornea of the side operated on offers a greater degree of resistance to destructive influences, so that a similar lesion in both corneæ produces in the normal eye inflammation and ulceration, but in the eye of the operated side little or no alteration.

The effects which result from section of the 5th nerve anterior to the Gasserian ganglion do not occur if the superior cervical ganglion has been extirpated before the operation or immediately after. And even when they have advanced to some considerable extent they may be arrested and reparation may take place if the ganglion is extirpated.—For the reparation of the inflammatory and ulcerative effects on the eye and lips it is not necessary to take any special precautions for protecting the parts.

The author is inclined to explain these results by the greater vascularity and increase of temperature which result from the extirpation of the superior cervical ganglion.—If the carotid on the side operated on be tied, then section of the 5th is followed by its usual results, even when the superior cervical ganglion has been removed.

THE EXTENT OF THE FIELD OF VISION.—Uschakoff (*Reichert and Dubois' Archiv*, 1870, 454—483) explains the differences between the statements of various observers as to the extent of the field of vision, by shewing (by means of an apparatus after the model of Aubert Förster's Perimeter) that the field of vision is different for eyes of different refractive power.

It is largest in the hypermetropic eye, smallest in the myopic, and of medium size in the emmetropic eye.

The differences are quite independent of the conditions of the pupil.

MOVEMENTS OF THE EYES.—A. Skrebitzky (*Centralblatt*, No. 5, 1871. Abstract from *Nederl. Archiv. f. Genees. en naturk. V.*) confirms the statements of Hueck which have been denied by Donders, that when the head is inclined to one side the eyes move in the opposite direction. With an inclination of 10° the eyes move through an arc of 1°. The amount of movement increases with the degree of inclination, so that with an inclination of 70°—80° the eyes move through an arc of 8·6°. These values are much smaller than those given by Hueck.

BREADTH OF VISION.—In opposition to the opinion of Hering, Woinow (*Arch. f. Opth.* XVI. I. 200) thinks that the eyes have the power of seeing as widely round when they are accommodated for near as for distant objects. When an object is looked at sidewise, the

obliquity of the eye nearest it is less than that of the other, and they become unequally accommodated, as is well shown by the coloured rays then observed round a slit in which a piece of selenite is placed and looked at in this manner.

SENSIBILITY OF THE RETINA.—Woinow (*Op. cit.* p. 212) observes that the different sensations which colours produce according to the part of the retina on which they fall, are not due only to fatigue, but to differences in the sensibility of different parts of the retina, the peripheral parts of which are blind to green as well as to red, green being perceived by them as yellow.

BINOCULAR VISION.—When an attempt is made to see as one picture, two surfaces or two objects of different shape, colour, or brightness, or with different backgrounds, a struggle takes place between the pictures presented to each eye. Woinow (*Arch. f. Ophth.* XVI. I. 194) finds that this is best marked when a single point in each object is looked at, when the light is bright, and the colours lively and contrasted. The *colour* of the combined picture is not the same as that of a true mixture of those of the single objects, as is seen when they are compared together by means of a prism.

An appearance of gloss is most readily observed when the surfaces are of very different brightness, as when one is white and the other black, or one yellow and the other violet. The *form* of that object to which the attention is directed is more distinct than that of the other.

MOVEMENTS OF THE IRIS.—Grünhagen (*Pflüger's Arch.* III. 440) finds, in opposition to Arlt and Donders, that irritation of the sympathetic acts with nearly the same rapidity on the iris and on the vessels of the ear, the former being affected in ·78 seconds and the latter in ·84 seconds after the irritation. He disputes the existence of a dilator muscle in the iris, and believes that dilatation of the pupil is due to contraction of the vessels of the iris.

INTRAOCULAR PRESSURE.—Monnik (*Arch. f. Ophth.* XVI. I. 49) by a new tonometer tested the pressure necessary to make a depression $\frac{1}{4}$ millimetre deep in the sclerotic by means of an ivory point. The pressure necessary was generally from 10 to 15 grammes, but sometimes was as little as 9, and sometimes as much as 14. It varied as much as 3 grammes in the same eye at different times, and seemed to increase with age. It was increased in congestive amblyopia and glaucoma, being as high as 45 grammes in the latter disease. In separation of the retina (Netzhautablösung) it was diminished to 6 grammes. It was unaffected by motion or accommodation. When the indications of the instrument were tested by comparison with a manometer inserted into the excised eyes of newly killed animals, it was found that 45 grammes pressure of the instrument was hardly equal to 185 millimetres of mercury. When a manometer was inserted into the aqueous and vitreous humours and water injected into the vitreous, the pressure rose in the aqueous as well, showing that the ciliary body, zonule and lens offer little resistance.

Hoppel and Grünhagen (*Op. cit.* p. 27) conclude from their researches that the sympathetic diminishes the intraocular pressure while the trigeminus on the contrary increases it, both by dilating the vessels of the choroid and iris, and also by causing fluid to filter more readily out of them.

Ear.

ACTION OF THE TENSOR TYMPANI.—A. Schapringer (*Wien Acad. Sitzber.* LXII. 2 Abth. 1870) has, under the direction of Helmholtz, made use of the faculty he possesses of voluntarily contracting his tensor tympani in investigating whether the tensor tympani effects an 'accommodation' of the ear. He finds that voluntary tension of the membrana tympani renders all notes below 70 vibrations inaudible, and those above this number weak. The higher tones, on the other hand, were more readily perceived and the partial tones more easily discriminated the more energetic the tension of the membrana tympani. Experiments as to whether under ordinary circumstances an accommodation exists give a negative result. He obtained interesting results as to the resonance tones of the ear.

Taste.

DEPENDENCE OF TASTE ON THE PART OF THE MOUTH IRRITATED.—Camerer (*Zeitsch. f. Biol.* 1870, p. 440) thinks that the fungiform and circumvallate papillæ alone are the organs of taste. He found that weak solutions of a sapid substance can be more readily tasted when they succeed water than when they come after a stronger solution. On the middle and under side of the tongue where fungiform papillæ are absent, no difference in the taste of water, sugar, sulphuric acid, quinine or salt could be distinguished. When the solutions were not too weak they were distinguished the more exactly, the greater the number of papillæ was with which they were brought in contact. The parts round a papilla have no sense of taste, but different tastes can be distinguished when one papilla alone is touched.

Circulatory System.

CONTRACTION OF VESSELS IN THE PIA MATER ON IRRITATION OF A SENSORY NERVE.—Nothnagel had found that irritation of a sensory nerve causes the vessels of the exposed pia mater to contract in rabbits which have not been narcotised. Riegel and Jolly have repeated his experiments on different kinds of animals, and find (*Virch. Arch.* LII. 2, 218) that this is due to pressure on the vessels of the pia mater from increased tension within the cerebrum during the cries or struggles of the animal, and is not a result of reflex action. When struggling was prevented by narcotising the animal no contraction took place, although both spinal and sympathetic nerves preserved their properties intact. Contraction is also produced by the flow of drops of arachnoidal fluid or blood over the pia mater, and still more strongly by cold water. The vasomotor nerves for the vessels of the pia mater only exceptionally run in the cervical sympathetic and its upper ganglion, and destruction or irritation of these generally produces no effect on the vessels.

WORK OF THE HEART.—Haughton, *Dublin Quart. Journ.* XCVII. 74.—Buchanan, *Lancet*, 1870, Vol. II. No. 20.

AMOUNT OF BLOOD IN THE BODY.—Brozeit, *Pflüger's Arch.* III. 353.

SOUNDS OF THE HEART.—Paton, *Dublin Quart. Journ.* XCIX. 93.

REFLEX EFFECTS ON THE CIRCULATION AND RESPIRATION FROM THE NASAL MUCOUS MEMBRANE.—Kratschmer (*Sitzber. d. Kais. Acad. d. Wissen. Math. Natur. Cl.* II. Abtheil. 1870, LXII. 24) has made experiments to determine the nature of the curious effects which are observed on the circulation and respiration when a pungent vapour, such as Ammonia, is held before the nostrils of a rabbit. As is well known the animal contracts its nostrils, the respiratory movements cease for several seconds in the state of expiration, and the heart likewise ceases to beat, or beats very slowly for some time. The author explains this result by reflex action from irritation of the nasal mucous membrane. He obtained the same result by blowing tobacco smoke up through the larynx into the nasal cavity. No such effect followed blowing smoke directly into the lungs. The effect of blowing smoke up through the larynx is not altered by section of the superior and inferior laryngeal nerves. He found that with the diminution of the cardiac beats there was slight rise in the blood pressure. If both vagi are cut no effect is produced on the heart when the vapour is applied, but the effects on the respiration and blood pressure remain as before. The blood pressure likewise rises in curarised animals. No effect follows irritation of the nasal mucous membrane after section of the trigemini within the skull. Hence the effects observed are to be explained by reflex action through the 5th nerve and the vagus as far as the heart is concerned. Mechanical and electrical stimulation of the nasal mucous membrane produce the same results as irritating vapours. Electric stimulation of the supramaxillary branch of the 5th causes a cessation of the respiration in the state of expiration. Irritation of other sensory nerves does not produce this effect.

REPLACEMENT OF BLOOD BY SALT SOLUTION.—Horwath (*Centralblatt*, 1870, p. 801) finds that when Na Cl solution of ·75 per cent. is allowed to flow into the central end of the divided abdominal vein of frogs, while the blood flows at the peripheral end according to Cohnheim's method, they always become œdematous and die before all the blood has been washed out, corpuscles being still seen by the microscope in the fluid issuing from the vein. The pressure used never exceeded 20 millimetres of mercury.

Bernstein (*Centralblatt*, p. 852) thinks that as the abdominal vein arises from a limited vascular district the blood cannot be completely washed out of distant parts of the body by fluid flowing into or out of it. He therefore advises that the thorax of the frog be opened, a canula inserted into one aortic arch, and salt solution allowed to flow through it towards the periphery, while the blood streams out of the open central end. A ligature is to be tied by a slip knot round the other aortic arch, so that no blood can pass through it till the fluid

issuing from the aorta is completely colourless, and shews no corpuscles under the microscope. The ligature is then removed from the other aortic arch, and salt solution allowed to pass through it for several minutes. The cut ends of the divided aorta are then ligatured, and the wound in the thoracic wall sewed up. Frogs deprived of blood in this way live for two or three days; and he thinks the œdema noticed by Horwath was due to the pressure under which the salt solution was introduced being too great.

Respiration.

INFLUENCE OF THE MEDULLA OBLONGATA ON RESPIRATION.—Schiff (*Pflüger's Arch.* III. 624) finds that section of the lateral columns of the medulla oblongata at the level of the first cervical nerves puts a stop to respiratory movements on the corresponding side of the body. Mere exposure of the medulla causes no change in the amount of air respired at one movement, but division of one-half of the cord, or of a lateral column, diminishes it by about one-third.

THE BLOOD-STREAM DURING INTERRUPTED RESPIRATION.—Dogiel and Kowalesky (*Pflüger's Archiv*, III. 489) find that the elevation of arterial pressure, which occurs when the respiration is interrupted, is always accompanied by a marked diminution of the velocity of the blood-current. During dyspnœa there is alternate quickening and slowing of the current.

RESPIRATORY MOVEMENTS.—Rosenthal (*Reichert and Dubois' Archiv*, 1870, 423, *and Centralblatt*, No. 3, 1871) tests the results arrived at by Nasse and Hermann (see Report in this *Journal*, Nov. 1870, p. 208) in reference to the question whether it is deprivation of Oxygen or accumulation of Carbonic acid that acts as the stimulus to the respiratory centres. The method adopted by Nasse was to inject salt solution ($0{\cdot}6\,{}_0/{}^0$) into the large vessels of the head, and thereby drive out the blood. Nasse concluded from his experiments that the deprivation of Oxygen does not stimulate the respiratory centres, for animals so treated soon cease to respire. Rosenthal shews that these experiments are unable to decide the question, as the cessation of respiration is in reality due to the rapid loss of the irritability of the nerve centres which ensues when they are deprived of their blood. He likewise made experiments with serum instead of salt solution, and found that the only difference between the injection of serum saturated with Oxygen and serum saturated with Carbonic acid was that the irritability of the respiratory centres was kept up a little longer in the former case than in the latter—a fact to be explained by the Oxygen preserving the vitality of the centres. The question, therefore, as to whether want of Oxygen or accumulation of Carbonic acid acts as the stimulus, must yet remain undecided as far as these experiments are concerned.

RELATIONS BETWEEN LESIONS OF LUNG AND BRAIN.—Brown-Séquard (*Lancet*, No. 1, 1871) had observed that after lesions of various parts of the brain in cats and guinea-pigs, the animals often died of

Pneumonia. Further experiments revealed that almost constantly ecchymoses and apoplectic extravasations occurred in the lungs, and bronchi, on crushing or section of the Pons Varolii. More commonly these results followed lesions of the crura cerebri and crura cerebelli. Less often they resulted from lesions of the medulla and spinal cord. The effect on the lungs is produced through the sympathetic and its spinal roots, and on the opposite side to the lesion. The results occur in whatever condition the lungs may be as regards respiration. In addition to hæmorrhages often local patches of anæmia resulted on lesions of the Pons. Œdema of the lung occurred in small patches on lesions of the medulla. Emphysema of the lung was also observed to result even when there were no respiratory movements. These results throw light on the common association of pulmonary with cerebral disease.

Respiration in Fever.—By adapting a mask to the face of fever patients, and connecting it with an apparatus for measuring the amount of air expired, Leyden (*Deutsch. Arch. f. klin. Med.* 536) found that the amount of air respired during the fever (recurrent, pneumonia, typhus) was to that when the fever was absent more than $1\frac{1}{2}$ to 1; the percentage of CO_2 in this was as 3 to $3\frac{1}{3}$; so that on the whole the excretion of CO_2 during the fever was increased by nearly one half. As increased combustion and excretion of CO_2 may occur in health without rise of temperature, he thinks the rise in fever is due to diminished evaporation from the skin. In those fevers where there is abundant sweating, as acute rheumatism, the rise in temperature is slight.

Diffusion of Gases between Arterial and Venous Blood.—N. O. Bernstein, under Ludwig's direction (*Ber. d. K. Sachs-Ges. d. Wiss. Math. Phys. Cl.* 1870, p. 124; *Centralblatt*, 1870, p. 669), took a portion of arterial blood from the carotid, and another portion after the animal had been nearly suffocated. The amount of gas in each was determined, and a portion of each was then placed in a vessel, and separated from each other only by a piece of cæcum, carefully cleaned with water and alcohol. After standing some hours they were again tested. An almost imperceptible amount of oxygen only passed into the venous blood from the arterial, and a somewhat greater, but still slight amount of CO_2, from the venous into the arterial. The number of experiments, however, was too small to allow of any decided conclusion being drawn from them.

Contractility.

Nature of Muscular Contraction.—Schmulewitsch (*Centralblatt*, 1870, 609) thinks that the contraction of muscle is produced by increased attraction between its molecules (elasticity). This increased elasticity is due to energy set free in the muscle during irritation being converted into it. He proves that one form of energy, viz. heat, can produce increased elasticity of muscle, by attaching one end of a muscle to a hook, and the other to a thread stretched over a metallic box. When the muscle was warmed the

thread gave a higher note, and the more the muscle was stretched at first the greater difference did warming produce in the note. If the muscle be kept at 28°C. the elasticity after rising for some time again begins to fall and the note becomes lower. The increase and diminution in the elasticity might be expressed graphically by a curve which differs in appearance from one produced by muscular contraction only in its slower rise or fall.

The elasticity becomes much altered when the muscle is fatigued. The increase and diminution in elasticity is not a vital process, as the same phenomena are observed in caoutchouc.

Duration of Muscular Contraction.—Klünder, *Arbeiten aus d. Kiel Physiol. Inst.* 1870, p. 107; *see report by Power in Brit. Med. Chir. Rev.*, Jan. 1870.

On the Nature of Muscular Force.—A. W. Volkmann (*Verhandl. d. k. s. Gesellsch. d. Wissen. zu Leipzig*, 1870. I. II. 57, 70). The author supports Weber's theory, that the extensibility and the contractility of muscle are both ultimately dependent on the same elastic properties.

Conditions of Muscular Fatigue.—A. W. Volkmann (*Pflüger's Archiv*, III. 372, 403) contributes a paper on the phenomena attending the fatigue of muscle. He finds that the time requisite for attaining the maximum amount of contraction (rapidity of contraction), as also the duration of the contraction, increase with the amount of fatigue, while the amplitude of the contraction diminishes. With a great degree of fatigue both duration and rapidity of contraction begin to diminish. He also confirms Weber's observation that there is a diminution of elasticity in the condition of fatigue.

Tetanus and electromotor power of Muscle.—Röber (*Reichert and du Bois Reymond's Arch.* 1870, 615) thinks that the diminution in the electromotor power of muscle after it has been tetanized is due to sarcolactic acid produced by the contraction. When frogs were poisoned with strychnia or picrotoxin, and one sciatic nerve cut, so that the muscles of that leg were not tetanized, their electromotor power was much less diminished than that of the other leg, and the slight diminution in the non-tetanized leg might be due to sarcolactic acid brought to it by the blood from the tetanized one. Injection of ¾ per cent. salt or 1·3 or 2·6 per cent. sugar solutions diminish the electromotor power of muscles both before and after tetanizing. Injection of salt solution containing lactic acid produced tetanus in a muscle and lessened its electromotor power one half. This could not be restored by washing out the vessels with salt solution alone, but could be so to a certain extent by salt solution containing carbonate of soda. This also increased the electromotor power when diminished by convulsions from picrotoxin.

Electricity from Muscular Contraction.—Mr Varley (*Spiritualist*, Feb. 15, 1871, and *Academy*, March 1) thinks that the current which passes from the clenched to the opened hand when

both are placed in water, is due to the acid perspiration squeezed from the pores. It is greatly diminished by previously washing the hands in ammonia and increased by dilute nitric acid. When one hand was washed in nitric acid, and the other in ammonia, squeezing either hand produced a current in the same direction, and when both hands were placed in water and a little acid dropped on one of them a current was produced without any muscular action.

F. MEGETIET AND S. HEPNER 'ON THE RELATIONS BETWEEN THE MATERIAL METAMORPHOSIS AND TENSION OF MUSCLE.' *Pflüger's Archiv*, III. 574, 578.

ON THE STIMULATION OF MUSCLES AND NERVES BY INTERRUPTED CURRENTS.—Th. W. Engelmann (*Pflüger's Archiv*, III. 33) has made numerous experiments in reference to the observations of Harless and Heidenhain, that under certain circumstances no effect is produced on a muscle when a constant current passed through its nerve is very rapidly opened and closed. E. adopted a method by which very rapid changes could be produced in the current, and he found that with a given rapidity of interruption no tetanus is produced. Each opening and closing of this rapidly interrupted current caused contraction just as in the constant current. He obtained similar results by applying the current to the muscles direct.

ON THE CONTRACTILITY OF THE CORNEAL CORPUSCLES AND SPACES. —Rollett (*Centralblatt*, No. 13, 1871) describes the appearance of the cornea of the frog under powerful induced currents. In the cornea which normally appears homogeneous, or exhibits here and there a vagrant cell or a glimpse of a stellate corneal corpuscle, there appear, after irritation, variously shaped figures which look like clefts in the tissue. Some which are roundish or elliptical resemble punched out holes, like the fenestræ of the fenestrated membrane of an artery—only smaller. These figures are different aspects of a canal system, in the nodal points of which the central nucleated portions of the stellate corpuscles lie. The margins of the canals are distinctly seen because the corpuscles on irritation contract away from the sides of the canals. These phenomena disappear after a time and again return on irritation. A distinct canal-system thus seems to be proved, and the contractility of the corneal corpuscles established against Engelmann and others.

CONTRACTILE GLANDS IN THE SKIN OF FROGS.—Engelmann (*Pflüger's Archiv*, IV.) demonstrates the presence of contractile glands in the frog's skin. These are under nerve influence, and may be made to contract (in the leg) by direct irritation of the distal end of the cut sciatic, or reflexly through this nerve. The gland nerves are not paralysed by curara.

The electro-motive properties of the frog's skin are related to this action of the glands, for when they are thrown into contraction there is a perceptible deviation of the cutaneous current.

Digestive System.

THE MOVEMENTS OF THE INTESTINE.—Mayer and von Basch (*Wien. Acad. Sitz. Math. Naturw. Cl.* 2. Abtheil. LXII., Dec. 1870) have made numerous experiments on dogs and rabbits under the influence of curara, with a view to determine the cause of the peristaltic action of the intestine. Compression of the thoracic aorta causes movements. So also stoppage of the artificial respiration. The influence of stoppage of the respiration is much more marked when the intestines are entirely severed from their connection with the central nervous system. When this is done the vessels of the intestines are no longer made to contract during the state of dyspnœa, and it is observed that the movements of the intestines begin when the dilated vessels are filled with venous blood, and that they again cease when the blood is oxygenated. Hence they conclude that the cause of the movements is the presence of venous blood. This is the explanation of the post-mortem peristaltic actions, for they begin when the vessels dilate and allow access of venous blood.

The action of the vagus after death in causing movements is explained in the same way. They explain the action of the splanchnic in arresting the peristaltic movements, by the fact that as it is the vaso-motor nerve of the intestine, irritation causes contraction of the vessels, and thus prevents the access of venous blood in quantity. They therefore do not consider that the splanchnic is in any special manner the inhibitory nerve of the intestine as Pflüger states.

ON PERISTALTIC ACTION—SPECIALLY OF THE INTESTINE.—Th. W. Engelmann and G. Van Brakel (*Pflüger's Archiv*, IV. 33) conclude that as in the ureters, so in the intestine, the movements are independent of nervous influence. A contraction occurring at one point propagates itself equally in both directions. No transference of movement however takes place from the small to the large intestine, or vice versa. The œsophagus, unlike the intestine, contracts only through the influence of the nervous system.

THE GLANDS OF THE STOMACH.—The researches of Ebstein (*Pflüger's Archiv*, III. 565) throw considerable doubt on the validity of the distinction commonly made between *mucous* and *peptic* glands of the stomach. His conclusions are based both on microscopical (*Archiv für Microscop. Anat.* VI. 515) and experimental observation on the stomach of the dog. He finds that an artificial gastric juice prepared from the mucous glands of the regio pylorica possesses the power of converting albuminoids into peptones in presence of Hydrochloric acid. The active principle is not therefore to be attributed to the so-called *peptic* cells, but rather to the cells common to both kinds of glands. He proposes the names *simple* and *compound* peptic glands in place of the terms *mucous* and *peptic* glands.

SECRETION OF BILE.—Schiff (*Reichert and du Bois Reymond's Arch.* III. 612) finds that the resistance to the passage of bile into the gall-bladder is less than into the intestine, so it all passes into the bladder

till it becomes distended or contracts. All the bile secreted can thus be removed by a fistular opening in the gall-bladder without tying the gall-duct, but when the fistula is closed it passes into the intestine. When all the bile is thus removed the amount secreted becomes much diminished, but again rises when the bile is re-injected into the intestine, stomach, veins, or under the skin. Ox-gall or the soda salts of bile-acids have a similar action. When much bile is injected into the intestine bile pigments are found in the urine. As former experimenters have withdrawn all the bile and not re-injected it into the intestine, their estimates of the amount secreted are too small, and Schiff reckons it at 1·3 to 3·2 gr. in an hour for every kilogramme of body weight. When the portal vein is slowly obliterated by a ring of thread put round it and gradually pulled upon, bile-colouring matter appears in the urine, but disappears in 3 weeks. When the salts of *bile acids* are injected into the blood, bile *pigment* is found in the urine, if the fistula be closed, but not if it be opened, shewing that the pigment in the urine is due to absorption of bile from the intestine. These changes in the amount of bile secreted are observed in sick as well as healthy dogs, and are not due to slowing of the pulse from the bile absorbed, as digitaline has no such effect.

FORMATION OF UREA IN THE LIVER.—E. Cyon (*Centralblatt f. Med. Wiss.* 1870, p. 580) estimated the amount of urea in blood drawn from the carotid of a dog and then passed one or more times, according to Ludwig's method, through the excised liver of the same animal. After passing through the liver the amount of urea it contained was increased; in one instance where the same blood was passed through the liver four times, it was more than double that contained in blood from the carotid, showing that a considerable amount is formed in the liver.

Temperature.

CAUSE OF FEVER.—Naunyn (*Reichert and du Bois Reymond's Arch.* 1870, 159) found that when heat was retained within the body of a dog by placing it in a warm atmosphere saturated with moisture the temperature in the rectum rose $7\frac{1}{2}^{\circ}$, and the urea rose in the next 24 hours from 6·7 to 9·76 grammes. This seemed to render it probable that retention of heat in the body was the cause of fever. Farther experiments however proved that this was not the case. He injected putrid matter into the veins of a dog and found that the excretion of urea was increased during the next hour or two, while the fever thus produced was still latent and before the temperature of the animal was increased. If fever had been dependent on retention of heat there should have been no increase of urea till the temperature of the body rose.

Senator (*Virch. Arch.* L. 354) thinks that the production of heat in the body is not increased by cooling the surface, and that no conclusions on this point can be drawn from observations made in the axilla alone, as variations in its temperature and that of the rectum do not run parallel. On cooling the naked body of a strong young man

he found that the temperature in the axilla rose $\frac{1}{30}$° th to $\frac{1}{8}$° C., while that in the rectum sank slightly. After several minutes that in the axilla began to sink and sank constantly even under the normal till the experiment was stopped. The slight diminution in that of the rectum was succeeded by a slight rise, and this again by a constant fall. He thinks that the contraction of the vessels of the skin produces a slight accumulation of heat at the periphery shown by the rise in the axilla, and that this passes gradually inwards till it produces the rise in the rectum. The body may be cooled so that the temperature rises in one axilla while it falls in the other. From these experiments he concludes that cold baths in fever diminish the temperature without causing any increased production of heat.

EFFECTS OF ABSTRACTION OF HEAT.—Horwath (*Wien Med. Wochensch.* 1870, No. 32) found that on cooling rabbits with snow and ice, the frequency of the pulse was diminished and the power of the vagus over the pulse and blood pressure destroyed at 23° C. The intestines and bladder were motionless and did not contract on electrical irritation. Peristaltic contractions could be induced in the former by dropping warm water upon them, but these disappeared on the reapplication of cold. Compression of the carotid had no effect on the blood-pressure and produced no convulsions, but inflation of the lungs diminished the pressure and altered the pulse-rate. These effects disappeared when the temperature of the animal was again raised.

DIMINUTION OF TEMPERATURE BY PAIN.—Horwath (*Centralblatt*, 1870, p. 546) had found that the temperature in the rectum of rabbits tied down to a board constantly sank sometimes 2° C., and this was not due to simple want of movement as it did not sink more than $\frac{1}{10}$° C. when they were simply put into a narrow box in which they could not move. He therefore imagined that it might be due to the *pain* caused by the cords and on pinching a little piece of skin 2 or 3 centimeters in diameter he found a similar sinking of the temperature.

DIMINUTION OF TEMPERATURE BY GAS IN THE INTESTINES.—Simons (*Inaug. Diss.* Bonn. 1870. *Brit. Med. Chir. Rev.* 223. Jan. 1870). Carbonic acid, carbonic oxide, hydrogen or air when injected into the intestines or subcutaneously cause an immediate fall of temperature, attaining its maximum in 2—6 hours and then gradually rising. The maximum fall is 10° C. Pulse and respiration were also somewhat slowed but they were not affected so much as the temperature.

INFLUENCE OF FOOD AND DIGESTION ON TEMPERATURE.—Vintschgau and Dietl have found (*Wien. Acad. Sitz. Ber. Math. naturwis.*—Cl. Bd. LX. Abth. II. p. 69) that warm food introduced into the stomach of a dog with a gastric fistula raised the temperature of the body, as observed by a thermometer in the rectum, while cold food depressed it. When the food was warmed to nearly the same temperature as the body, the temperature fell in the course of two or three hours about $\frac{1}{100}$th of a degree, and then rose $\frac{1}{5}$° to $\frac{1}{2}$° C. above its original one. The temperature of the rectum sank and rose in a parallel manner.

NORMAL AND PATHOLOGICAL TEMPERATURE.—Jacobson (*Virchow's Archiv*, LI. 275) in conjunction with Bernhardt and Leyden has carried on his researches on this subject. Some results have already been detailed in a former number of this *Journal* (Second Series, No. IV. May, 1869). With reference to the question whether fever heat is due to retention of heat, *i.e.* an equalisation of temperature between warmer and cooler organs within the body, or to absolute increase of temperature, Jacobson and Leyden found that in fever resulting from injection of pus, or croton oil inflammations, the temperature increases both in the liver and in the rectum, which could not be the case if there were merely an equalisation of temperature. The authors, however, do not think that the source of the increased heat is the local inflammatory processes (as held by J. Simon and and Billroth), for they found, that though the temperature of the inflamed limb is higher than that of the other, yet it is always lower than that of the abdomen, rectum or vagina. They likewise find that there is no difference in temperature between a pleura inflamed by injection of ammonia or acetic acid and the pleura of the other side.

EFFECTS OF CEREBRAL LESIONS ON THE TEMPERATURE OF THE BODY.—Bruck and Günther (*Pflüger's Archiv*, III. 578) find that an elevation of temperature is more surely produced when puncture is made in the anterior part of the pons or posterior part of the medulla than after separation of the medulla from the pons. Hence they conclude that the elevation of temperature is to be considered as the effect of irritation, rather than paralysis of an inhibitory centre as Tscheschichin supposes.

INFLUENCE OF THE NERVOUS SYSTEM ON THE TEMPERATURE AND CIRCULATION.—R. Heidenhain (*Pflüger's Archiv*, III. 504) was led to researches on this subject from experiments made with a view to determine the temperature of the brain in different conditions. He found that the brain always possesses a higher temperature than arterial blood, and that this difference is markedly increased on stimulation of sensory nerves. There is a fall of temperature in the arterial blood, but this does not take place if the spinal cord is severed from the medulla oblongata. Along with the fall in temperature there is increase of the blood-pressure. He believes that the fall of temperature is not due to mere disturbance of the circulation effected through the medulla. He finds (in opposition to Dogiel and Kowalesky) that with the rise in arterial pressure the pressure in the veins is also increased and *also the velocity of the blood-current*. Hence he concludes that as the fall of temperature is not due to slowing of the blood-current through the influence of the sensory nerves on the medulla, it must be due to increased radiation from the surface. He finds a confirmation of this view in the fact that, when the body is immersed in a cold bath, the internal temperature rapidly sinks on irritation of sensory nerves, while the reverse is the case in a warm bath where no radiation from the surface is allowed.

SEPTICÆMIA.—Albert and Stricker (*Medic. Jahrbüch der K. K. Gesellsch. d. Aertzte*, 1871, Heft 1) find that the increased temperature produced by the injection of pus into the veins may be due to irritation produced by the local wound or embolism, and not to a specific poison. When a vein was simply tied the temperature rose during the operation, fell after it, and again rose in the third hour afterwards. When anything was injected, the temperature rose in the first hour and did so after a certain rhythm, the rise being at first regular, then quicker, and then again suddenly slow. Rigors occurred when the rise began or when it became quicker. Injection of ½ per cent. Na Cl solution or spring water produced the same effect as diluted pus. Pure pus caused a quicker rise, but this also could be imitated by injecting water holding a number of starch granules in suspension. When the crural vein is connected by a canula with the crural artery, so that blood flows out of one into the other, a rise in temperature is also produced.

Injection of fresh defibrinated rabbit's blood into the peritoneum of a dog produced vomiting, purging, and laboured breathing, just as pus had been found by Weber to do when injected into the thorax. When an animal was bled, and the blood of a feverish animal transferred into it, its temperature *fell* and did not rise so quickly to the normal as when healthy blood was transferred.

INDEX TO VOLUME V.

W.

Y.

Z.

CAMBRIDGE: PRINTED BY C. J. CLAY, M.A. AT THE UNIVERSITY PRESS.

/r

88.

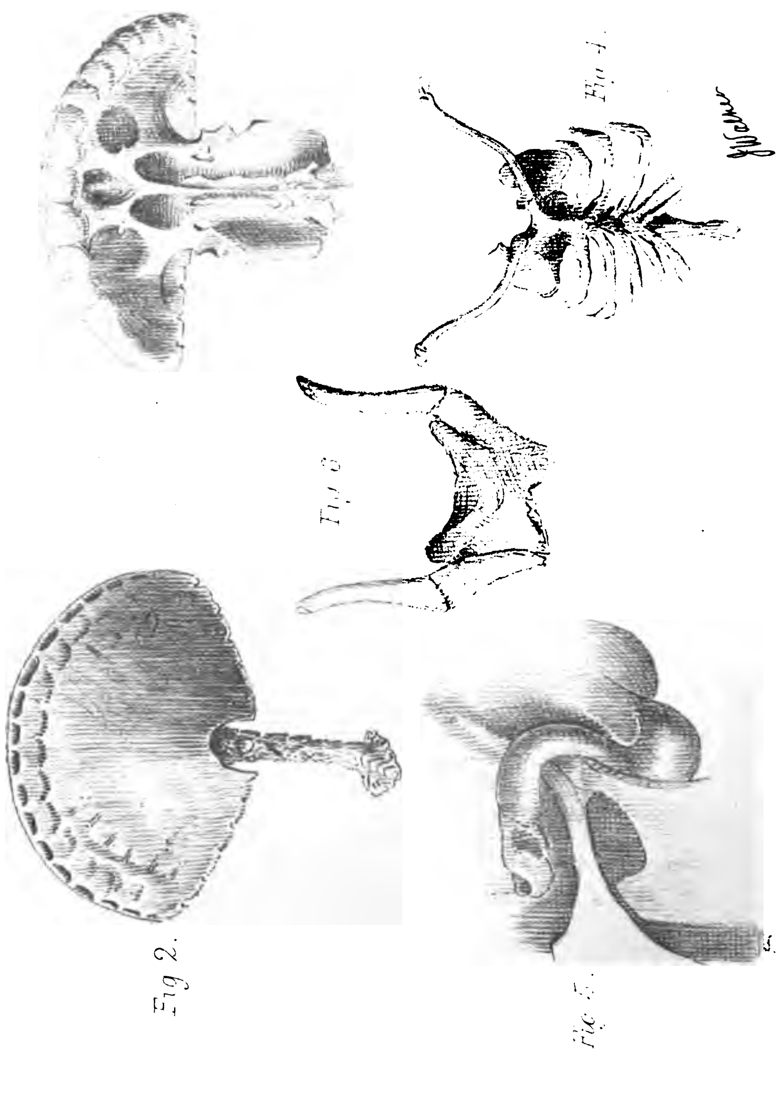


Fig 3.

Fig 4.

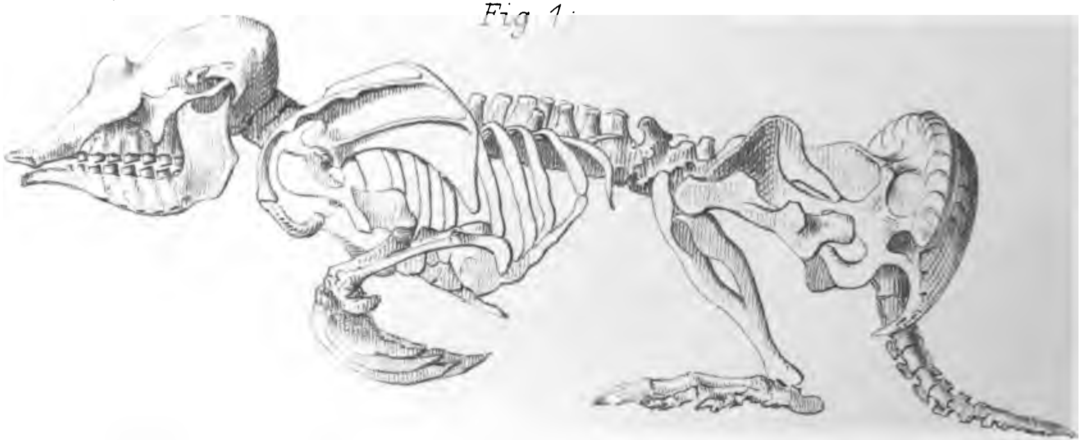
Fig 6.

Fig 2.

Fig 5.

CHLAMYDOPHORUS.

Fig. 1.



J. Walker

CHLAMYDOPHORUS.

Fig. 1.

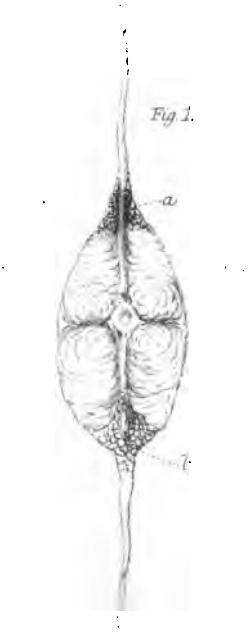


Fig. 2.

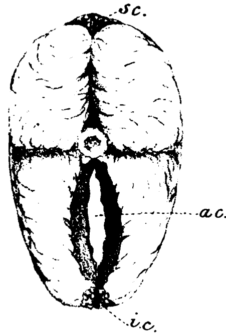


Fig. 3.

Fig. 4.

Fig. 5.

Fig. 6.

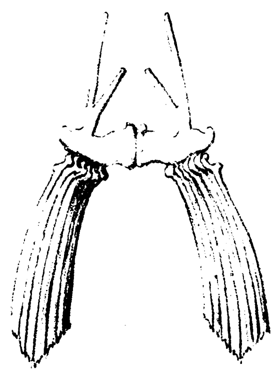


Fig. 7.

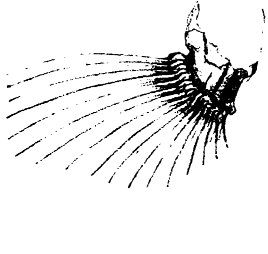


Fig. 1.

Fig. 2.

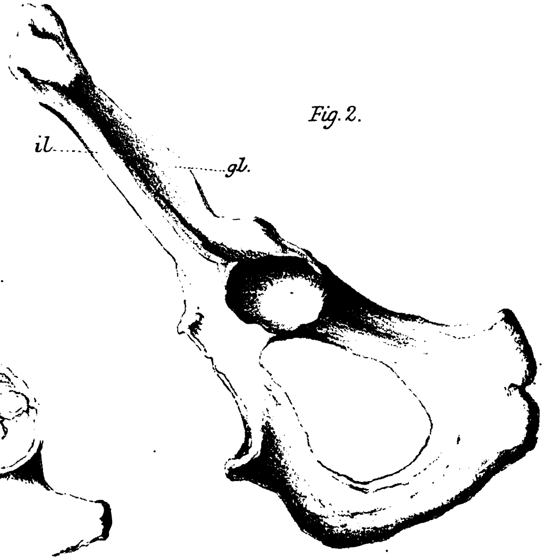


Fig. 3.

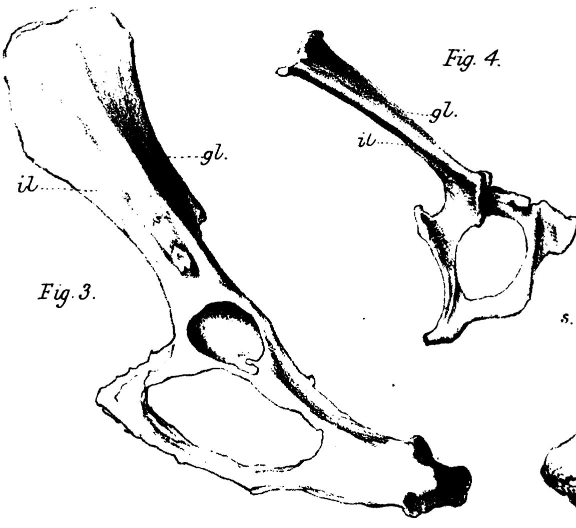


Fig. 4.

Fig. 5.

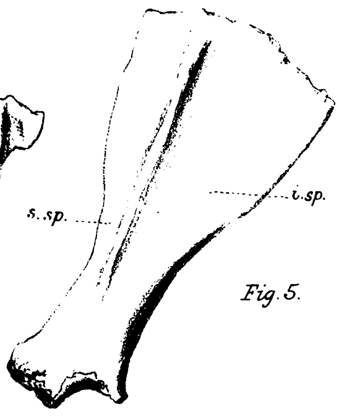


Fig. 6.

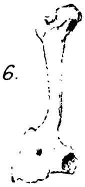
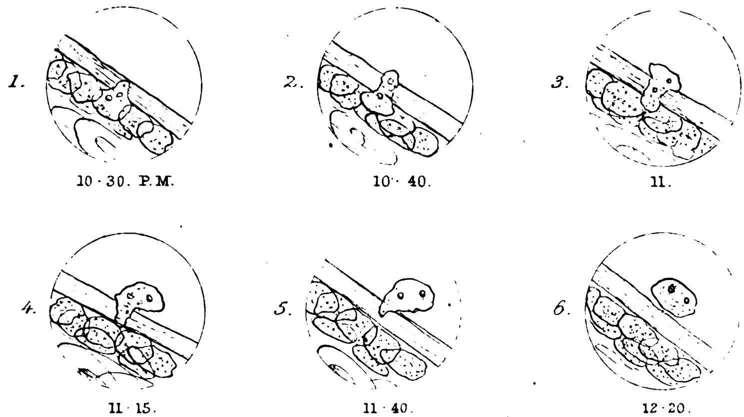
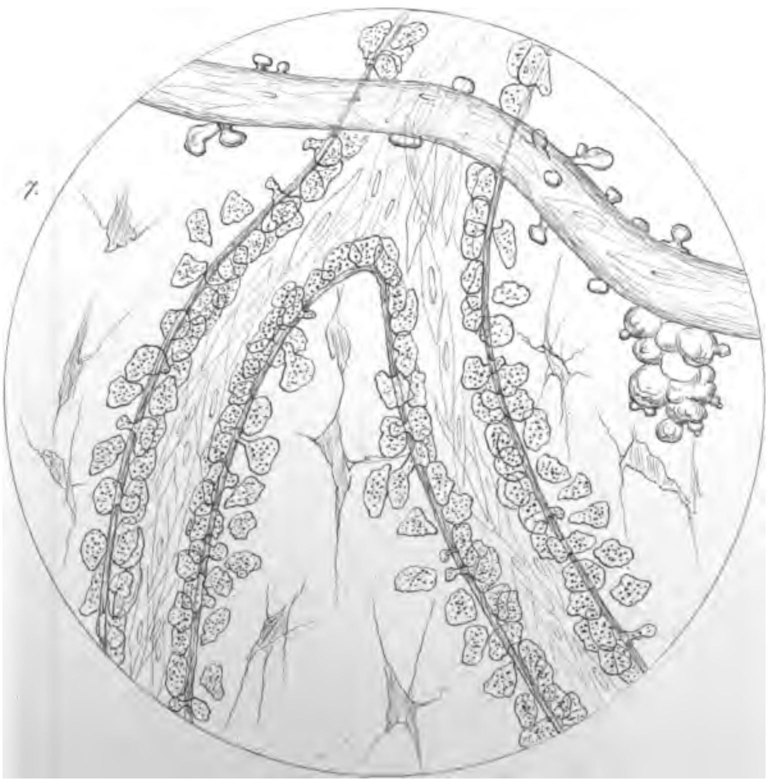


Fig. 7.



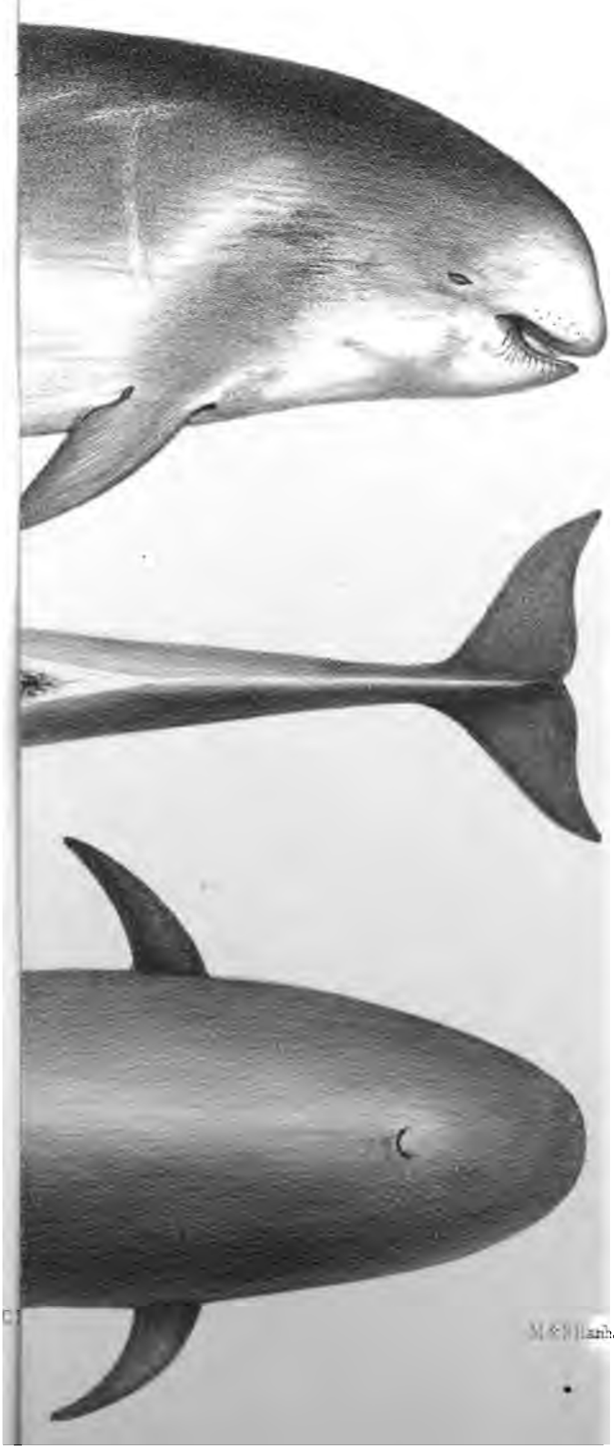
Stages of the Migration of a White Blood Corpuscle through the Wall of a Vein.
(Mesentery of the Frog.)



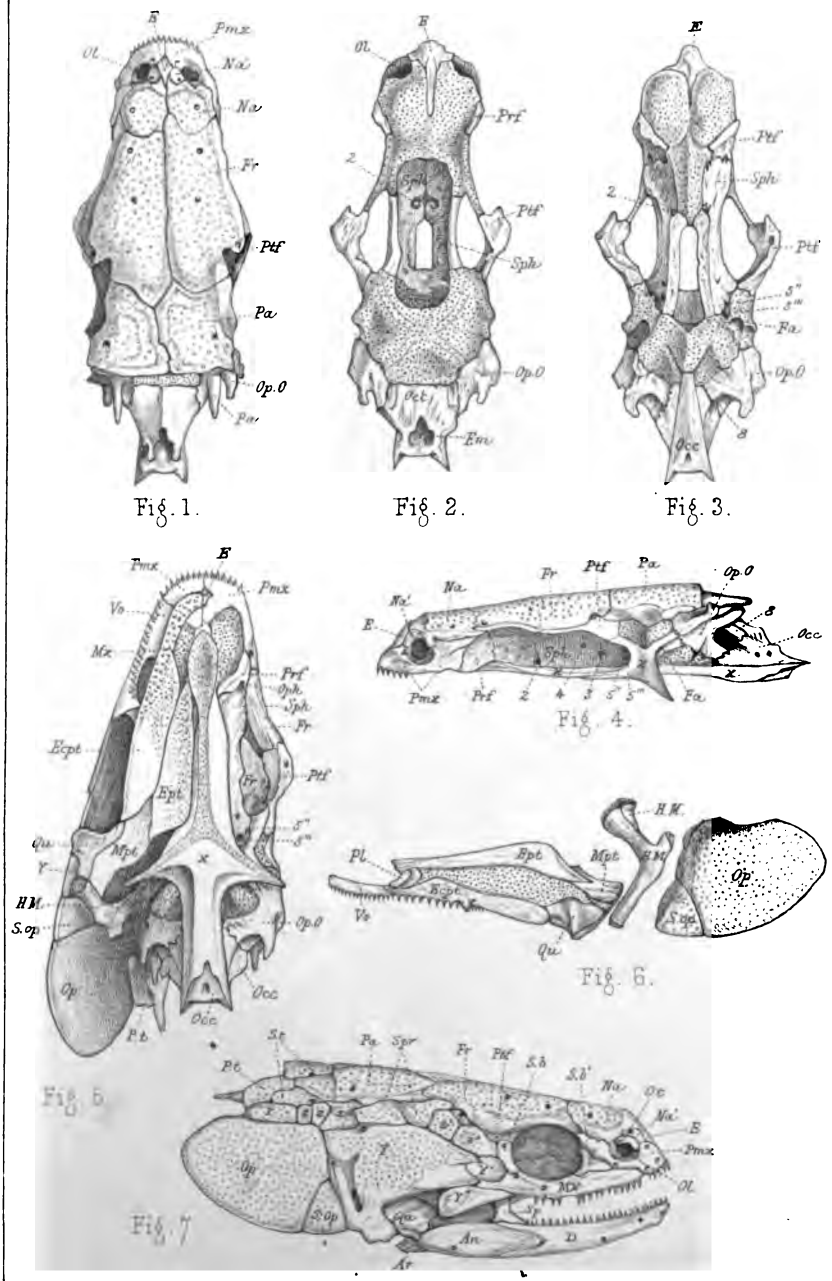
Migration of White & Red Corpuscles in the Tadpole.

R. Caton. M.D. delt. W H Mc Farlane, Lithr. Edinr.

Pl.V.

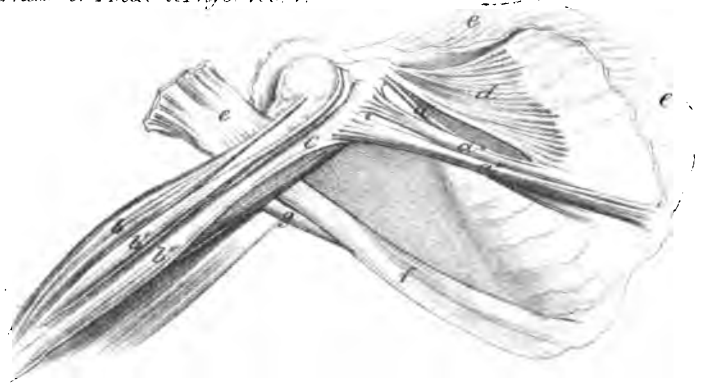


M.&N.Hanhart imp



Skull of Polypterus Bichir.

R. H. Traquair M. D. delt. W. H. McFarlane Lithr. Edin



ANATOMICAL ASPECT OF RIGHT SUPERIOR APPENDICULAR PART,
SHEWING THE EPIGASTRIC MUSCLE (CHONDRO CORACOID OF WOOD.)

Fig. 1.

a, a″, a‴, Epigastric slips. b, b′, b″ Glenoid acromial and coracoid factors of Biceps. c, Coraco brachialis. d, Pectoralis minor. e, e, e, Pectoralis major. f, Latissimus dorsi. g, Teres major.

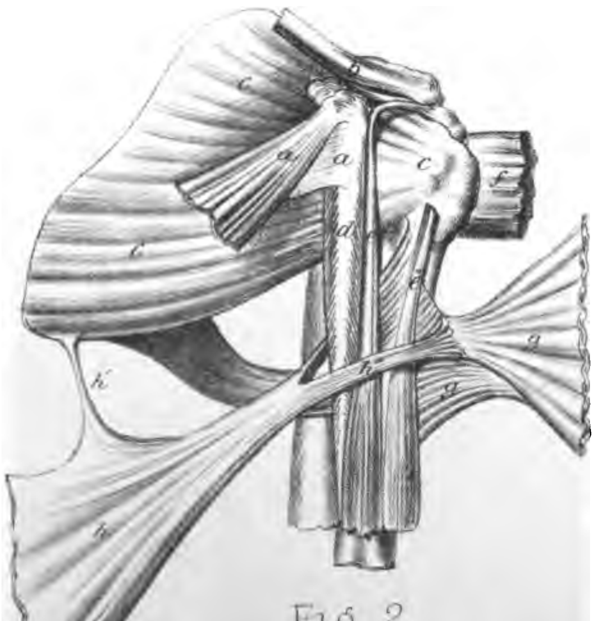


Fig 2.

ANATOMICAL ASPECT OF LEFT SHOULDER MUSCLES

a, a, Pectoralis minor, shewing coracoid and fascial insertions. b, Clavicle. c, c, c, Subscapularis. d Coraco brachialis. e, e″ short and long heads of Biceps. f Portion of Deltoid (reflected.) g, g, Pectoralis major. h, Latissimus dorsi. h″, Achselbogen. h′, Slip of origin of Latissimus from inferior angle of Scapula

Fig. 3,

LEFT FORE LIMB OF SQUIRREL.

a, Costo-humeralis simulating the Epigastric slip of human subjects.

a', An Epicostal muscle (first described by Prof. Wood)

b, Latissimus dorsi and dorsal Panniculus shewing Dorso epitrochlearis &c.

c, c, c, Pectoralis major.

Fig. 4,

LEFT SHOULDER, (HUMAN).

a', a', Chondro epitrochlearis
a, Pectoralis major.
b, Deltoid.
c, Fascia covering Biceps.

Fig. 5,

RIGHT SHOULDER &c. (HUMAN)

a, Pectoralis major.
a' a', Chondro epitrochlearis.
a'', Capsular origin.
o, Internal humeral condyle.

Fig. 6.

LEFT SHOULDER (HUMAN)

a, a, Chondro epitrochlearis.
a', Slip from Coracobrachialis.

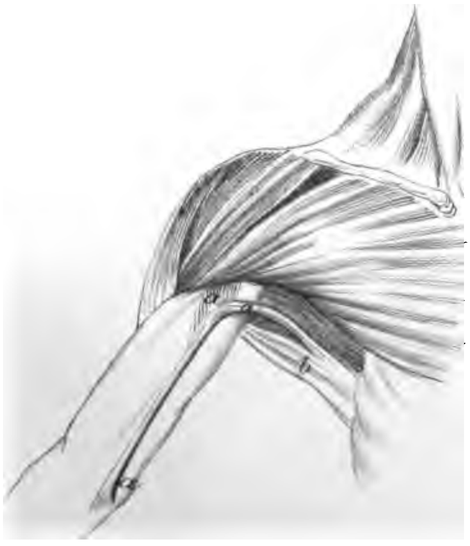


Fig. 7.

RIGHT SHOULDER (HUMAN)

a a, a, Dorso epitrochlearis.
b, Latissimus Dorsi.

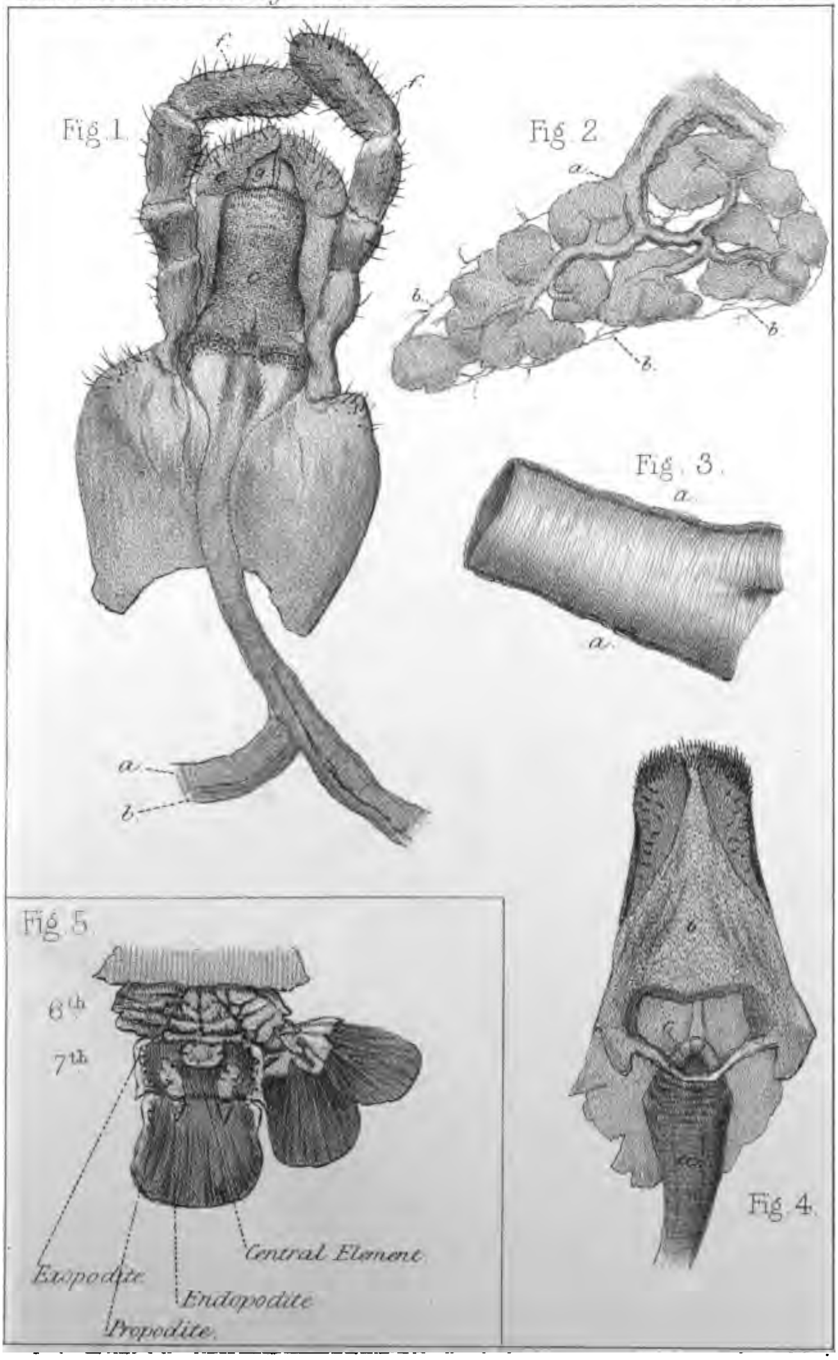


Fig. 1.
f.
f.
a.
b.
Fig. 2.
a.
b.
b.
b.
Fig. 3.
a.
a.
Fig. 4.
Fig. 5.
6th
7th
Central Element
Exopodite
Endopodite
Propodite